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No. 5

SIR DAVID GILL, K.C.B., F.R.S., PRESIDENT, in the Chair.

Edmund Sewall Manson, Jun., Assistant Professor of Astronomy,
Ohio State University, Columbus, Ohio, U.S.A.; and
Robert Charles Slater, M.A., Assistant Master, Charterhouse,
Godalming,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

George Bruford, Tolland House, Shoot-up-Hill, Cricklewood,
N.W. (proposed by S. A. Saunder);
Reinold John Jauget, Captain, Mercantile Marine, 53 Chatsworth
Street, Sunderland (proposed by T. W. Backhouse); and
James Hopwood Jeans, M.A., F.R.S., formerly Fellow of Trinity
College, Cambridge, Professor of Applied Mathematics,
Princeton University, New Jersey, U.S.A. (proposed by
Arthur R. Hinks).

One hundred and one presents were announced as having been received since the last meeting, including, amongst others:—

Algiers Observatory, Carte photographique du Ciel, coordonnées rectilignes, tome vi., fasc. 2, presented by the Observatory;
Ch. André, Les planètes et leur origine, presented by the author;
Harvard Observatory Annals, eight vols. and parts, presented by the Observatory;
Lick Observatory Publications, vol. viii. (Keeler's photographs of nebulae), presented by the Observatory; Paris

Observatory, Atlas Photographique de la Lune, fasc. 10, presented by the Observatory; R. Patxot Jubert, Observaciones de San Feliu de Guixols, presented by the author; B. O. Peirce, Theory of the Newtonian Potential Function, presented by Mr. Frank Robbins; A. Stanley Williams, Zenographical Fragments, part 2, presented by the author.

Astrographic Chart; 36 charts, presented by the Royal Observatory, Greenwich; 20 charts, presented by the San Fernando Observatory, 14 by the Tacubaya Observatory, and 22 by the French Minister of Public Instruction, from the Bordeaux and Toulouse Observatories.

On Correlation and the Characters of Variable Stars: in reply to Professor Karl Pearson. By H. C. Plummer, M.A.

1. The paper* in which Professor Pearson has discussed some of the statistical relationships to which different classes of variable stars may be subject has natural claims to our respectful attention. Much work in modern astronomy, whether expressly inspired by the evolutionary point of view or not, is of the nature of classification. Professor Pearson has done so much to provide the necessary theoretical basis for studying the correlation of classified characters that we should be grateful to him for dealing with some of the problems connected with variable stars which are of this kind, and thereby drawing more general attention to a branch of astrophysics which is of the greatest interest and importance.

At the same time it seems to me that some of Professor Pearson's results stand in need of serious qualification, being open to a very simple line of criticism (see §§ 3, 4), which he has apparently overlooked. If this be conceded, these results must lose much of their value, and no very great confidence can be placed in certain ulterior speculations which have been based on them. This may be thought a harsh judgment. But if it can be shown that in this instance Professor Pearson, possibly misled by a certain want of familiarity with the subject of variable stars, has made a not altogether judicious use of his own machinery, it will surely be better to vindicate the general methods, even at the expense, if need be, of the particular results.

Perhaps an example will serve to explain, if it does not justify, the feeling that such a critical attitude is necessary. In the case of long-period variables, Professor Pearson finds a correlation between the light-range and maximum light of seventy-nine stars which is little short of unity (.9996). Now if this means anything at all, it should mean that the light-range is almost rigorously a linear function of the maximum light. But if this be so, a similar relation ought to hold between the maximum light and the minimum light. Now this is not so. Hence it is impossible to

* *M.N.*, lxix. p. 128 (1908 Dec.).

draw any conclusion of value from the figure found for the correlation, the magnitude of which, it may be suggested, is fully accounted for by the simple fact that the minimum light is very small in comparison with the maximum light for this class of stars. The biologist may perforce be content with the knowledge that a relationship exists: the astronomer is not accustomed to rest content until he knows something about the nature of the relationship. He will certainly not be attracted by modern methods in statistics if they lead to results from which no useful deduction can be made, as they appear to do in the example quoted. It is therefore of great importance that we should scrutinise carefully the application of a form of statistical calculus which itself is no doubt capable of doing useful service.

2. One point of a general character must be considered before we come to any question of statistics. Professor Pearson insists, not for the first time, that it is "absolutely needful to use the true physical variable amount of luminosity, and not the merely physiological variable magnitude." By what canon, it may be asked, is he able, as he seems to be, to distinguish *a priori* between these two classes of variables? One is purely a mathematical function of the other, and it is readily conceivable that the result of a statistical discussion would be to show what particular function of a variable would be most appropriate for use in a particular problem; that, in fact, this might be the most useful outcome of such a discussion. But in Professor Pearson's paper the discrimination appears as a matter of *a priori* judgment, and not at all as a tentative suggestion to be verified by experience. This view of his attitude may be mistaken, but it seems probable that there is a real preconception on his part with regard to this point, and that it has prejudiced both his treatment of the statistics and his interpretation of the results.

However this may be, the phrase quoted above seems to have been expressed a little unfortunately. The astronomer is perfectly ready to admit the detrimental effect of "physiological" influences. But there are two reasons why the use of the word is not very happy in this particular case. The determinations of magnitude can be made photographically, and not visually at all; indeed, they have been so made by Professor S. I. Bailey, whose results for the ω Centauri variables are employed by Professor Pearson. Again, it is no question of the errors in the determinations which may arise from this—or any other—source. For such errors would be reproduced in the deduced luminosities, and the latter are accepted without the slightest suggestion that they are in any way inaccurate.

On the other hand, the expression "true physical variable" may easily cover a false assumption. In dealing with variable stars, it must be borne in mind that in no case is there complete certainty that we possess the true explanation of the light-fluctuation. The use of a term like "obscuration" begs the whole question. Even in the case of the Algol variables, the eclipse theory can claim no more than a high degree of probability—a very

high degree, no doubt, but one falling distinctly short of certainty. If we consider the light-curves for the Algol and for the antalgol types, we might expect to find some analogous features in the causes underlying the two classes; by accepting the eclipse theory for the former, we apparently exclude such a possibility. Of course we know that Algol is a spectroscopic binary, but as much may probably be said of every short-period variable. This argument has therefore little weight, especially when we notice* that the reconciliation of photometric and spectrographic observations of Algol is far from being the certain and simple matter we could desire. But if there is room for so much as the slightest suspicion in the case of Algol itself, what is to be said of the other types discussed by Professor Pearson?

When the physical cause underlying the variability is entirely unknown, both magnitude and luminosity are merely measures of some physical change. To discover which is the more appropriate measure is a matter of experiment, and the experiment must be repeated with every fresh line of discussion. On the commonly accepted theory of Algol variables, Professor Pearson is no doubt justified in choosing the luminosity as the more appropriate variable. But were the type of variation that which would be caused by the varying thickness of a stellar atmosphere producing uniform and general absorption of the star's light, it is easily conceivable that the star's magnitude would be the more appropriate variable, as directly connected with the thickness of the atmospheric envelope. And this is merely a random suggestion of one possibility.

3. We can now turn to the point in the theory of statistics which Professor Pearson has apparently overlooked. In this we are taking advantage of a clue which he has himself given us, for it is he who has drawn our attention to the importance of spurious correlation. We have, writing $\sigma(x)$ for the standard deviation of x , and $r(x, y)$ for the coefficient of correlation between x and y ,

$$\begin{aligned}\sigma^2(x - y) &= \sigma^2(x) + \sigma^2(y) - 2r(x, y)\sigma(x)\sigma(y) \\ \sigma^2(x) &= \sigma^2(y) + \sigma^2(x - y) + 2r(y, x - y)\sigma(y)\sigma(x - y) \\ \sigma^2(y) &= \sigma^2(x) + \sigma^2(x - y) - 2r(x, x - y)\sigma(x)\sigma(x - y)\end{aligned}$$

The form of these equations suggests a simple geometrical interpretation. Consider a triangle ABC whose sides are $AB = \sigma(x)$, $AC = \sigma(y)$, and $BC = \sigma(x - y)$. It follows that

$$r(x, y) = \cos A, \quad r(x, x - y) = \cos B, \quad r(y, x - y) = -\cos C.$$

Now one angle of a triangle at least must be less than 60° . Hence one at least of the three coefficients of correlation must be greater than $\cos 60^\circ = 0.5$, and this number is supposed to indicate appreciable correlation when the quantities are independent. But if one of them, say $r(x, y)$, is small, which will be the case if x

* Cf. R. H. Curtiss, *Astrophys. Jour.*, xxviii. p. 150; Schlesinger and Curtiss, *Publ. Allegheny Obs.*, vol. i., No. 5.

and y be in no way related, then A will be nearly 90° , and either B or C must be 45° or less, so that one at least of the other correlation coefficients, say $r(x, x-y)$, will exceed numerically $1/\sqrt{2}$, or 0.707 . This explains how quite a large coefficient of correlation can be obtained which is wholly spurious.

But this is not all. It is evident that

$$\sin B < \sigma(y)/\sigma(x),$$

and therefore, if $\sigma(x) > \sigma(y)$,

$$r(x, x-y) = \cos B > \sqrt{\{\sigma^2(x) - \sigma^2(y)\}}/\sigma(x).$$

Let B_0 be the greatest value of B consistent with this inequality. Then we see that if x and y represent any two series of numbers whatever, and a perverse imp were to tamper with the order of the two series without changing the numbers themselves in any way, so as to make the coefficient of correlation between x and $x-y$ the smallest possible, he could not by any possibility make it less than $\cos B_0$; and unaided nature cannot be expected to reduce it even to this limit.

4. Let us compare, then, the values of $\cos B_0$ in the several cases with the correlations obtained by Professor Pearson.

Cluster Variables (91 Stars).

$$\sigma(L_2) = .0137, \quad \sigma(L_1) = .0051, \quad \sigma(L_2 - L_1) = .0134$$

$$\left. \begin{array}{l} B_0 = 21^\circ 51', \quad \cos B_0 = .9281 \\ r(L_2 - L_1, L_2) = .9283 \end{array} \right\}$$

Algol Type (57 Stars).

$$\sigma(L_2) = 168.16, \quad \sigma(L_1) = 60.08, \quad \sigma(L_2 - L_1) = 109.33$$

$$\left. \begin{array}{l} B_0 = 20^\circ 56', \quad \cos B_0 = .9340 \\ r(L_2 - L_1, L_2) = .9959 \end{array} \right\}$$

Algol Type (54 Stars).

$$\sigma(L_2) = 15.79, \quad \sigma(L_1) = 6.15, \quad \sigma(L_2 - L_1) = 10.14$$

$$\left. \begin{array}{l} B_0 = 22^\circ 55', \quad \cos B_0 = .9211 \\ r(L_2 - L_1, L_2) = .9837 \end{array} \right\}$$

Long-Period Variables (87 Stars).

$$\sigma(L_2) = 15.756, \quad \sigma(L_1) = 3.446, \quad \sigma(L_2 - L_1) = 14.870$$

$$\left. \begin{array}{l} B_0 = 12^\circ 38', \quad \cos B_0 = .9758 \\ r(L_2 - L_1, L_2) = .9763 \end{array} \right\}$$

Long-Period Variables (79 Stars).

$$\begin{aligned} \sigma(L_2) &= 15.582, & \sigma(L_1) &= 5.15, & \sigma(L_2 - L_1) &= 15.357 \\ B_0 &= 1^\circ 54', & \cos B_0 &= .9995 \} \\ r(L_2 - L_1, L_2) &= .9996 \} \end{aligned}$$

5. A simple inspection of these figures suggests that Professor Pearson has placed a false interpretation on his results. The high correlations obtained do not correspond with any close physical relationship between maximum light and light-range, but are merely expressions of the fact that the variability of the maximum light is very much greater than the variability of the minimum light. This fact itself seems significant, and, taken by itself, would suggest that the efficient cause behind the variation of a star's light is in general of an additive rather than of a subtractive or "obscuring" kind. No undue stress need be laid on this remark, but it will be remembered that Professor Turner,* in attempting to bring the Sun into line with long-period variables, was led to identify Sun-spot maximum with star-light maximum,—not star-light minimum, as Professor Pearson seems by his language to assume as a matter of course. However this may be, it is evident that spurious correlation has played so large a part that, with the possible exception of the Algol variables, no room is left for the influence of any physical correlation between maximum light and light-range. Even in the case of the Algol variables, the figures not only prove that the correlation need not be particularly large, but even suggest that it is surprisingly small. At any rate, if sensible correlation exists, the method followed tends rather to conceal it than to bring it to light. It is unnecessary to follow Professor Pearson in the inferences which he has based on the large values found for $r(L_2 - L_1, L_2)$.

6. So far as the relationship between range and maximum brightness is concerned, it does not appear that the observed magnitudes furnish an index which is necessarily inferior to that supplied by the deduced luminosities. A trial has been made in the case of the short-period variables of the ω Centauri cluster, with the following result:—

Cluster Variables (91 stars).

$$\begin{aligned} \sigma(m_2) &= 0.234, & \sigma(m_1) &= 0.197, & \sigma(m_2 - m_1) &= 0.293 \\ B_0 &= 57^\circ 21', & \cos B_0 &= .5395 \} \\ r(m_2, m_2 - m_1) &= .7540 \} \end{aligned}$$

The apparent correlation is much less than that which results from the luminosities, but the certainly spurious element is also much less, so that room is left for the effect of a small but possibly real correlation. The example tends to show that star-magnitudes

* *M.N.*, vol. lxiv. p. 550.

are not so obviously unworthy of direct statistical treatment as Professor Pearson has implied.

7. If we except the Algol type stars, the figures for the correlation between period and light-fluctuation seem to lead to a similar conclusion. For the results obtained by Professor Pearson and Professor Turner (p. 138) give—

Type.	No. of Stars.	$r(P, F)$.	$r(P, m_1 - m_2)$.
Cluster . .	91	.457	42
Long-period . .	87	.049	
„ . .	79	.201	

For the cluster variables the results are practically the same; for the long-period variables the correlation between period and fluctuation is too small to be of any serious importance. The correlation between period and magnitude range has not been calculated for the long-period variables. But it is unnecessary to do this in order to see that we have no evidence that there is any advantage in using the light-fluctuation rather than the magnitude range in dealing with either of these two classes of variable stars.

8. There remains for brief consideration the apparent exception in the case of the Algol stars. Without excluding any of the 57 stars employed, Professor Pearson finds that

$$r(P, F) = .4109, \quad \eta(P, F) = .57, \quad r(P, m_1 - m_2) = .26$$

The regression line is curved, and the correlation coefficient is not a satisfactory measure, therefore, of the relationship between P and F , nor, presumably, of the relationship between P and $m_1 - m_2$. The nature of the curved regression line is not very clearly indicated by the data, only 15 stars having a period of more than 4 days, and it is a little difficult to judge how far the results are affected by the inadequacy of the material, and by the natural selection which depends upon the circumstances favourable to the detection of variability. The results of S. Beljawsky, quoted by Professor Pearson, refer to a different class of stars, with a quite different range of periods, and have no bearing whatever on the present question. The point of interest suggested by the present discussion of the Algol stars is, in view of its bearing on the eclipse theory, that apparently the period is *positively* correlated with light-fluctuation. The “ready explanation” which Professor Pearson suggests without formulating, and then criticises, has not occurred to me, and it does not seem to require two or three pages of mathematical reasoning to see that the increase of light-fluctuation with period is altogether surprising, if it be real. For if we take a given system and, *ceteris paribus*, increase the period and therewith the mean distance, the result will certainly be to diminish the fluctuation, unless we observe from a point exactly in the plane of the orbit. Professor Pearson’s suggestion, to meet the difficulty, is that a large orbit implies a large eclipsing body. The idea is an interesting one, and it is not impossible that some light might

be thrown on the question by a careful study of a large number of light-curves. Perhaps an alternative process of physical development may be suggested. Consider a system with a relatively small orbit and short period. The components may be supposed at first to be of approximately equal luminosity. With the lapse of time, the operation of tidal action will gradually increase the period and the distance between the bodies. Meanwhile, unless the components are of equal mass and similar constitution, a difference of luminosity may be expected to develop between the components, and the effect of increasing contrast may suffice to counterbalance the effect of increasing distance on the light-fluctuation. If such an explanation were valid, it is again possible that confirmation might be supplied by a study of the elements of a large number of stars, but at present we have scarcely enough material to settle such a question.

9. As already stated, the facts by which such conjectures are prompted are much obscured by the uncertainties to which random and natural selection may give rise. Moreover they have been interpreted in the light of the eclipse theory, and it is quite conceivable that some stars have been included to which this theory does not apply. Even a superficial inspection of the cases which are commonly admitted to a list of Algol variables is enough to suggest that a distinct advantage would be gained if a clear definition of what constitutes an Algol variable could be adopted. Such a definition must be framed with the object of separating the true Algol type from the β Lyræ type. This is not an easy thing to do perhaps, and it will probably be necessary to have an intermediate class in which the doubtful cases can be kept either temporarily or permanently. But whatever may be the difficulties in the way of a precise classification, it is hard to see, for example, why such a star as V Puppis should be associated with β Persei and separated from β Lyræ. The most promising line of attack is by the properly coordinated application of both the spectrograph and the photometer to members of both types. Coordination has hitherto been lacking, so that it has happened that when a spectrographic orbit has been worked out, the corresponding photometric data have either been entirely absent or of an accuracy not sufficient to inspire complete confidence. It is true that the field of such work is restricted and hampered by the faintness of the stars and the early type of spectra characteristic of the Algol class. But I may perhaps express the belief that more will be done by the most careful study of a few typical examples than by the mere accumulation of a mass of uncoordinated material and the indiscriminate volley-firing, so to speak, of a statistical method.

On the Testing of Paraboloidal Mirrors.

By Rev. C. D. P. Davies, M.A. (Plates 14, 15.)

1. *Introduction.*

In view of the fact that the Society's publications contain nowhere any connected account of the testing of paraboloidal mirrors, it seems desirable to give the theory and its practical application.

2. *References.*

The following references may be found useful. They are given in chronological order.

(1) Léon Foucault, in vol. v. of *Annales de l'Observatoire de Paris (Mémoires)*, 1859, published an account of his testing mirrors at their centre of curvature. This paper forms the basis of the modern method of testing.

(2) Henry Draper, M.D. (1864), gives a fragmentary description of portions of the subject in vol. xiv. of *The Smithsonian Contributions to Knowledge*. This is reprinted in vol. xxxiv.; see specially section "d," pp. 13-19.

(3) H. A. Wassell gives the fullest and best description hitherto published in the form of letters to the *English Mechanic*. See Nos. 887, 890, 894, 899, 902, 907, 911, 962, 1078, 1089, and 1129, ranging from April 1882 to November 1886.

(4) Dr. A. A. Common, F.R.S., makes some remarks on the matter in *R.A.S. Memoirs*, vol. 1. pp. 149-158.

(5) A. Woolsey Blacklock, M.D., gives a series of five excellent and reliable letters in Nos. 1579, '81, '83, '85, and '88 of the *English Mechanic* (1895).

(6) Professor G. W. Ritchey writes on the subject in vol. xxxiv. of *The Smithsonian Contributions to Knowledge*; see especially pp. 30-38 (1904).

3. *Principle of the test.*

The essential feature of the operation of testing a parabolic mirror consists in the fact that it is conducted at the centre of curvature, and not, as in the days before Foucault, at its principal focus.

The mirror is illuminated by a point of light placed as close as possible to the axial line of the mirror, and at a distance from the mirror equal to its radius of curvature. Under these circumstances an image of the pin-hole is formed at the same distance from the mirror as the pin-hole, and as far on the one side of the axial line as the pin-hole is on the other side of it. The pinhole and its image should always be brought as near to each other as is practically possible. On placing the eye at the image of the pin-hole the face of the mirror is seen illuminated, and can be scrutinised in detail.

4. *Apparatus.*

For effecting this scrutiny three pieces of apparatus are required. These are (1) the lamp, (2) the measuring machine, and (3) the support for the mirror.

5. *The Lamp.*

In the first of these, the lamp, the most essential condition is that the flame should be bright. The lamp that I have always used is a paraffin one, and the light has always been found amply sufficient. Acetylene would probably be found advantageous, but there is no real necessity for it.

An incandescent electric light is to be carefully avoided, owing to the fact that the light proceeds from a filament, which is practically length without breadth, whereas it is a surface or disc of light that is required.

6. *Its size.*

The lamp should also not be larger than is absolutely necessary, for the sake both of avoiding unnecessary heat and also of enabling both the light and the eye to be as near as they can be brought to the optical axis of the mirror. This is perfectly easy to accomplish, at least in the case of mirrors of ordinary focal length, such as 3 or 4 feet or more; as a distance of 3 or 4 or even 5 inches between the source of light and the eye is quite permissible, and does not in the least impair the efficiency of the test. Reflecting prisms have been proposed, and I believe sometimes used in this connection. In the case of small mirrors of 12 inches focal length or less it is quite likely that they might be required, but otherwise they introduce wholly needless complication.

7. *The chimney.*

Around the chimney at the height of the flame, and to a sufficient distance both above and below it, so as to fairly darken

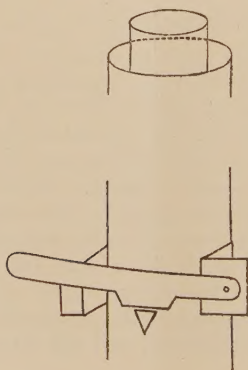


FIG. 1.

the room, there is fixed a circular sheath or outer chimney of thin sheet metal (see fig. 1).

8. *The triangular hole.*

In this is cut an opening in the form of an equilateral triangle, the sides being about a quarter of an inch long, the apex of the triangle being directed downwards, and being at the exact level of the brightest part of the flame.

9. *The lever.*

Around the back half of the tube is riveted or hard soldered a band of thin metal about half or three-quarters of an inch wide. It is produced tangentially from the two sides of the tube until it is flush with the front of the latter, when its two ends are bent outwards at a right angle, being cut off at about half an inch from the angle. This half inch on either side forms on the one side a surface to which is pivoted the movable lever or shutter for reducing the aperture of the triangular hole, while on the other it forms a bearing surface for the arm or handle of the lever.

Fig. 1 will render all this clear. It is essential that the lower edge of the lever, where it comes down in front of the triangular hole, should be in absolute contact—should, in fact, press gently “spring-tight” on the tube at this place. Otherwise, when the hole is reduced to a star, the star will not be “clean.” To ensure this, the bottom angle of the triangle is made as smooth and sharp as possible. It is a good plan to bevel the two sides of the angle upward from the inside with the finest possible triangular file, and to similarly bevel the bottom edge of the occulting lever downwards from its outside, thus bringing the two edges into the closest contact.

This admirable device for complete control of the size of the hole we owe to Wassell. It is greatly superior to the arrangements adopted by Draper, Common, and others, consisting of various-sized holes in the chimney, capable of being brought into position by rotating the same; or of perforated slips sliding into a slot in the chimney; for with these devices there is great danger of the distance from hole to mirror being changed in making adjustments, the measurements being vitiated accordingly.

10. *View-angle of flame.*

Before leaving the subject of the lamp, I may remark that if the wick is flat, it is best to orientate the triangular hole so that its plane is inclined at an angle of 45° to that of the flame. This secures the greatest depth and consequent brightness of flame, while the latter is at the same time not unduly narrowed.

11. *The measuring machine.*

I now come to the second piece of apparatus, the measuring machine. The essential feature in this consists of a “shutter” employed for cutting off the cone of light from the mirror close to

the focus and the eye. It must be capable of two movements at right angles to each other, one movement towards and from the mirror, the other at right angles to the former, *i.e.* cutting horizontally into the cone of light. These two movements must, moreover, be under complete control. They must be capable of being as minute as may be desired, and of being stopped and remaining at any desired point. In addition, the movement parallel with the axis must be accurately measurable.

12. *The shutter.*

How this is accomplished is shown in figs. 2-7. Here A is

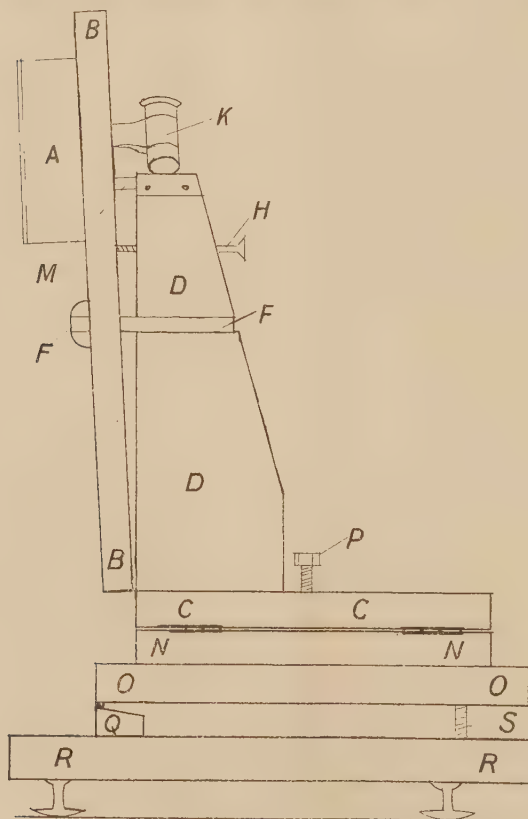


FIG. 2. (Scale $\frac{1}{4}$.)

the shutter. It is shown in figs. 2-6, and the way in which it is screwed to B is shown in elevation in fig. 5.

Fig. 2 shows the apparatus in elevation as seen from the direction of W on the plan, fig. 6. Fig. 4 is the same from the direction X,

fig. 3 from Y, and fig. 5 from Z. Fig. 7 is the plan of the bottom turned upwards.

The shutter A is best made of sheet zinc. The particular form given to it is so designed that the right eye may be brought close up to its outer vertical edge. It should be mentioned that the observer sits on the side Z, that is, fig. 5 represents the side facing

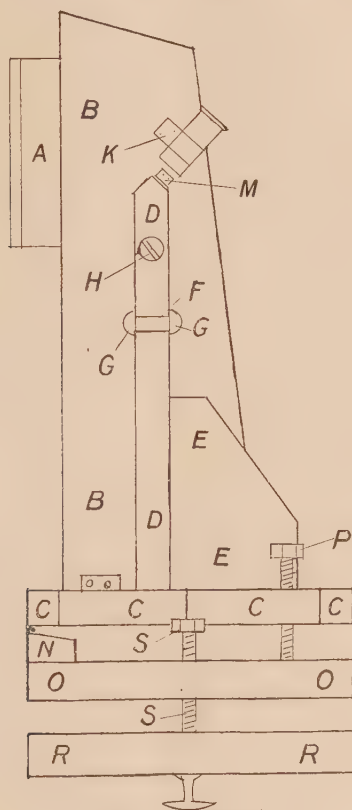


FIG. 3. (Scale $\frac{1}{4}$.)

him. The nose falls into the recess to the left of the shutter-edge. The edge should be very clean and smooth. A good method of finishing it off is to rub it on an oilstone till all trace of roughness is obliterated. In use it should be frequently dusted by passing the finger down it.

13. *The stand.*

The board B to which the shutter A is screwed is hinged at the bottom to the board C; screwed to C or mortised into it is the

upright piece D. This is rendered rigidly firm by another upright piece E screwed to it and also into C. An elastic band F is put around D, and opposite to it in B a slot G is cut. The band is pulled through the slot, and a short peg of wood is slipped through the end of the band to keep it strained and in its place.

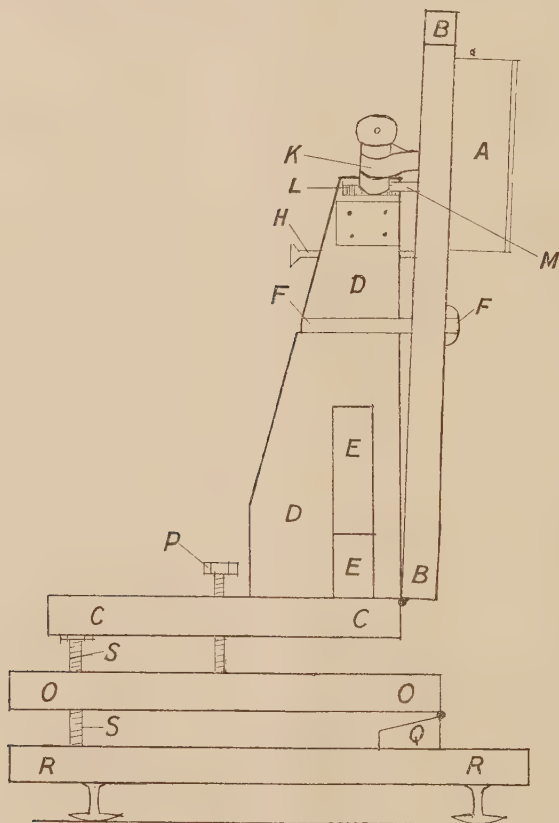


FIG. 4. (Scale $\frac{1}{4}$.)

14. *The wood screw.*

A long (but not too stout) wood screw H is passed through D above the band F. While not so loose as to be shaky, it should be capable of being screwed in or out by the finger and thumb. Its pointed end being filed off flat, is caused to bear against the board B. The bearing point on B should be protected by nailing a small piece of tin over it. By turning the bearing screw in or out it is evident that the shutter A may be advanced or withdrawn with

precision, and that it will remain where left. The best number of threads for the screw is ten to the inch.

15. *The eyepiece.*

In a clip K (in my apparatus it is made of sheet zinc) is inserted a low-power positive eyepiece, which is focussed on a scale L fixed to the bevelled top of the piece D.

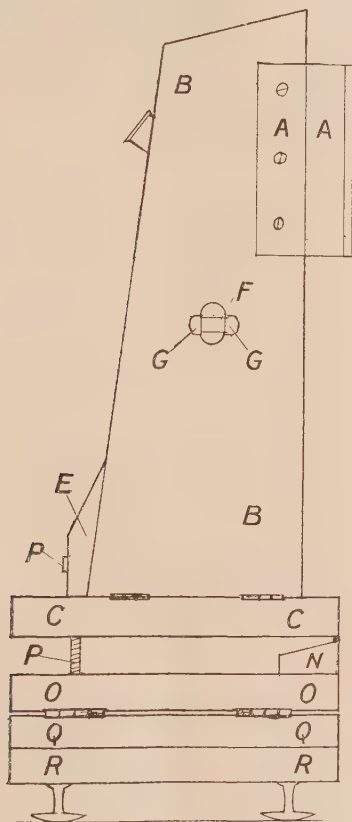


FIG. 5. (Scale $\frac{1}{4}$.)

16. *The scale.*

The scale is one of an inch divided into 100ths.

17. *The index.*

Fixed to B is a small index M (shown in figs. 2, 3, and 4, but not in fig 6) which, as B is actuated by the screw H, slides along

the scale. It should be of such length that the index-point is in the centre of the field of the eyepiece. The position of the shutter A, as compared with its position when the index is at the zero of the scale, can then always be read off.

18. *The bottom boards.*

The piece C is hinged along the side W (see fig. 5) to a strip N which is nailed or screwed to the piece O. Working in a nut let into the under side of C is the set-screw P bearing on O. It is evident that the effect of any turning of the set-screw is a move-

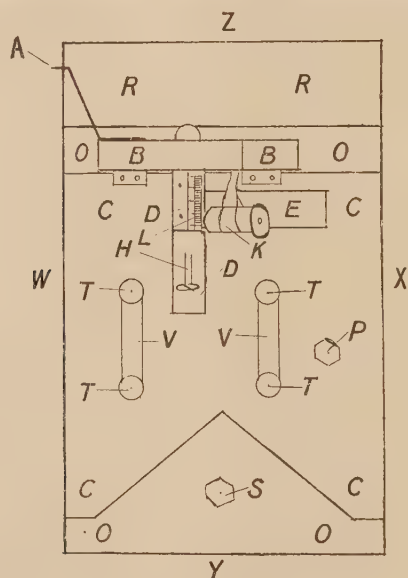


FIG. 6. (Scale $\frac{1}{4}$.)

ment of the shutter in a line parallel to the sides Y and Z in fig. 7. This motion of the shutter is that which is at right angles to the axis of the mirror when the apparatus is in use—see section 11. The piece C has a portion towards the end Y—of triangular shape in my own apparatus—cut out of it to admit of easy access to the head of the set-screw S. The piece O is attached by hinges and a strip Q to the base-board R, the hinges and strip being in this instance on the side or end Z. Working in a nut in the under side of O is the set-screw S bearing on the surface of the base-board. This screw gives a motion to the shutter A parallel to the sides W and X, that is, parallel to the axis of the cone of light reflected from the mirror—see section 11. Small pieces of tin should be fastened to O and R to take the bearing of the ends of the screws P and S respectively. Fig. 7 shows the bottom of the base-board and the

position of the three feet on which the whole stands, two at the end Z and one at the end Y. These feet are simply three japanned cupboard knobs or apron hooks.

The screws H and S impart similar motions to the shutter. The reason for them both will appear later on (see section 33).

At the four points marked T in fig. 6 drill holes of about $\frac{1}{2}$ -inch diameter straight through all the pieces C, O, and R, and lace them all through twice over with a long piece of $\frac{1}{2}$ -inch drapers' elastic, V, sufficiently straining it the while.

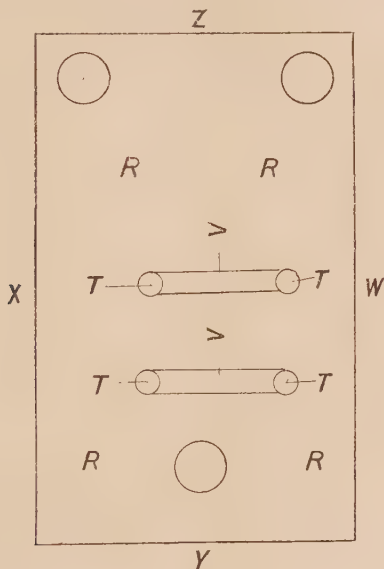


FIG. 7. (Scale $\frac{1}{4}$.)

Tie its two ends in a knot underneath. The purpose of the elastic is to keep the pieces O and R pressed against the ends of the set-screws P and S, and also to keep the whole apparatus taut when it is lifted.

19. *An improvement.*

With an important exception, this apparatus is practically Wassell's. I found that it much needed improvement in one particular. The two screws in his machine corresponding to P and S worked against the table on which the apparatus stood, serving also the purpose of legs. Besides the constant unnecessary friction occasioned by their working on wood, there was a continual tendency in the apparatus to shift slightly with the turning of the screws; and, added to all this, when the piece B was pushed out more than usually far by the screwing in of H, I have known the whole instrument tilt bodily over. All this is obviated by the introduction

of the pieces O and R, and by the prolongation of R in the direction Z as shown in fig. 2.

20. *The support.*

There remains yet the third item—a support for the mirror. Take a board A (fig. 8) about two inches wider than the largest speculum which it is proposed to test, about three or four feet long, and of sufficient thickness to support the mirror firmly and securely. At four or five inches from one end of this, nail or screw perpendicular to A another piece B of the same width as A, and of height equal to its width. To increase its rigidity add struts or brackets C fixed to A and B, one on each side. The mirror is placed with its back resting on the face of B. By far the best

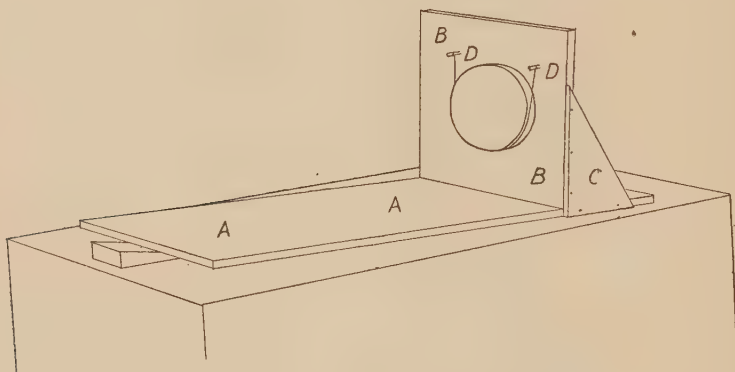


FIG. 8.

manner of supporting it in this position is to sling it. For this purpose screw a couple of rather long and strong screws D D into B to hold the band in which to sling the mirror. The band may be of webbing, or even of good blind-cord. Perhaps the best of all is a piece of thin sheet brass or zinc. The advantage of slinging is that any flexure arising from the support is precluded; blocks of wood or other rigid forms of support are best avoided. To give the necessary slant backwards the stand is, in testing, always placed on a surface on a lower level than that of the table on which the lamp and measuring machine are placed, so as to give the piece A an inclination of about 10° upwards. The back of the mirror should then lie gently, but firmly and free from all shake, against B. The required adjustment in the inclination is given by a wedge under the near end of A. A tap on the face of the mirror with a small pad of cotton wool held between the finger and the thumb will at once show if all is right. If not right, the mirror will perceptibly rock or rattle. Note carefully that cotton wool or some equivalent should be used. Touching the face of a mirror with the hand is apt to cause scratches, and, before testing, is attended by the

additional drawback that a temporary mound is caused on the surface of the glass by the warmth of the hand.

21. *Curves of Mirrors (three classes).*

In dealing with concave mirrors we encounter three classes of curve.

These are distinguished by the lengths of the radii of the different zones. In the first class the length of radius decreases, that is, the curvature increases, in passing from the centre to the rim. In the second class the length of radius, and therefore the curvature, is the same for all portions of the surface. In the third class the length of radius increases, that is, the curvature decreases, in passing from the centre to the rim. The second of these classes contains only one member, the sphere, and a mirror conforming to it is termed a "spherical" mirror. For reflecting parallel rays to one focus a "parabolic" mirror is required, and such a mirror belongs to the third class, which is therefore the class with which we are principally concerned. With the first class we have little or nothing to do, except that we must be able to recognise it at once should it be found in the course of testing. The way of doing so will be explained later on.

22. *The theory of "correction."*

In figuring and testing specula the spherical figure (class 2) is regarded as the zero figure, and the process of converting it to the parabolic figure (class 3), which process is technically termed "correction," consists in so manipulating the polisher that the radii of curvature become gradually shorter as the centre is approached, until the proper comparative length for each zone is reached. If the radii of the inner portions are too long the mirror is said to be "under-corrected." If the contrary is the case, the mirror is said to be "over-corrected."

23. *Longitudinal aberration.*

The proper (paraboloid) depth of curve is indicated by a formula giving the "longitudinal aberration" of any given zone on the mirror.

24. *The formula.*

This formula is a very simple one. For an analytical proof of it I am indebted to the kindness of Mr. H. C. Plummer; and, with his sanction, the proof is given as an appendix to this paper.

The complete formula in the notation adopted by Mr. Plummer is

$$\frac{D^2}{4R} + \frac{D^4}{32R^3}$$

in which

D = diameter of zone

R = radius of curvature.

Now take $r = \text{radius of zone}$

then $D^2 = 4r^2$

and $D^4 = 16r^4$

whence $\frac{D^2}{4R} + \frac{D^4}{32R^3} = \frac{r^2}{R} + \frac{r^4}{2R^3} = \frac{r^2}{R} + \left(\frac{r^2}{R}\right)^2 \times \frac{1}{2R}$.

25. Second term negligible.

The second term of the complete formula is so small as to be nearly always negligible. Take, for instance, even such a case as that of the rim of a 12-inch mirror of 3 feet focal length. Here $r = 6$ and $R = 72$, whence for the exact aberration we get $0.5 + 0.0017$. The value of the second fraction is well under $\frac{1}{500}$ inch. We are therefore justified in omitting it in all but the most exceptional cases.* When desired, its value is most easily found by utilising the form of it last given above, viz. the square of the numerical value found for $\frac{r^2}{R}$ divided by $2R$.

26. Draper's formula.

It will be noted that in his appendix Mr. Plummer gives an alternative formula for the aberration $= \frac{D^2}{2R}$ under the name of Draper's formula. This formula applies to the case where the lamp and pin-hole move with the shutter towards and away from the mirror.† It is curious that although Draper gives this formula in his paper, the woodcut accompanying it shows the lamp standing on the table, i.e. not moving with the knife-edge, in which case the formula would give only one-half of the aberration.

27. Outermost zone taken as zero.

The formula $\frac{r^2}{R}$ gives the amount of aberration measured in a direction away from the mirror with the centre of curvature of the centre of the mirror, as zero. In practice it is better to measure towards the mirror from the focal point of the outermost zone, the rays at this point being inclined at a greater angle, and the point

* Note a misprint in Common (*Memoirs*, vol. 1. p. 151), where the brackets in the numerator have been omitted. It should read not

$$\frac{\frac{1}{2}D^2}{R} \text{ but } \frac{(\frac{1}{2}D)^2}{R}.$$

† My attention having been called to a paper by Professor W. J. Hussey on "The Longitudinal Aberration of a Parabolic Mirror" contained in the *Publications of the Astronomical Society of the Pacific*, vol. xiv., No. 87, p. 179, issue of Dec. 1, 1902, which I have just seen, I am glad to find that he and I are in perfect agreement on this point.

therefore more sharply determined. The method of calculation will be understood from the following example. Take the case of a 12-inch mirror of 8 feet focal length, the surface being divided into six concentric zones an inch wide. Taking the mean radius of each zone, we have $r=0\cdot5$, $1\cdot5$, $2\cdot5$, $3\cdot5$, $4\cdot5$, and $5\cdot5$, while $R=192$.

From this, by the formula

$$a = \frac{r^2}{R}$$

we obtain for the aberrations $\cdot0013$, $\cdot0117$, $\cdot0325$, $\cdot0638$, $\cdot1055$, $\cdot1575$.

Taking these in reverse order, and subtracting each from $\cdot1575$, the aberration of the outermost zone, we have

Zone.	r .	a .
I.	$5\cdot5$	$\cdot0000$
II.	$4\cdot5$	$\cdot0520$
III.	$3\cdot5$	$\cdot0937$
IV.	$2\cdot5$	$\cdot1250$
V.	$1\cdot5$	$\cdot1458$
VI.	$0\cdot5$	$\cdot1562$

28. *Arrangement of apparatus.*

In actual testing, the arrangement is as follows:—On a small three-legged, solid and preferably rather heavy table, place the lamp and measuring apparatus side by side, the former on the right and the latter on the left, and beyond them, at twice its focal length, swing the mirror in its support, the support standing on some surface of suitable height (see sect. 20), and, with the triangular aperture in the metal chimney fully open, adjust the mutual distance and position of the support and the table so that the image of the triangular hole falls in focus upon and is perpendicularly bisected by the shutter edge. If it is day-time the room must be darkened. It is also imperative that the room should have a *solid* floor of some sort, *i.e.* not an ordinary wooden floor on joists, which would render the measures utterly nugatory. To avoid errors arising from personal discomfort the observer should always be seated, and be in a position of ease.

29. *Adjustments for zonal test.*

The full process of testing a mirror consists of two main divisions, called respectively the “zonal test” and the “shadow test.” Of these we take first the zonal test. The initial object will be to get the shutter exactly into the focus of the zero or outside zone.

30. Diaphragms.

For effecting this we must place the proper diaphragm on the face of the mirror. Of such diaphragms we shall need a complete set, corresponding with the zones into which we have decided to divide the mirror, and for which we have calculated the parabolic aberrations.

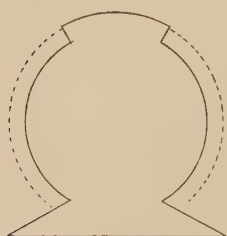
31. Proper width of zones.

I find that an inch is a very convenient width for mirrors of from 6 to 8 feet focal length, varying proportionately for other focal lengths.

32. Making and fixing diaphragms.

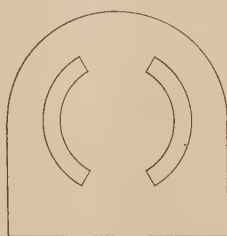
The diaphragms are easily cut out of cardboard. They should be just large enough to cover the face of the mirror, or perhaps better, to protrude a 16th of an inch all round, as it is then easy to tell when they are centrally placed. They may be square or rounded at the top to coincide with the upper half of the circumference of the mirror, but they should be straight at the bottom to ensure their resting squarely and correctly on the ledge provided for them. This ledge may be a piece of wood lightly nailed to the piece B (fig. 8) just below the mirror, or simply two nails of sufficient length tapped into B, or any other device that suggests itself. Diaphragms hung on the mirror or hitched to it in any way are to be avoided. Not only are they tiresome to adjust, but in adjusting them there is always a chance of moving the mirror.

The following figures show examples of the diaphragms. In the figure of zone I the dotted line indicates the edge of the mirror. In other respects the figures speak for themselves.



Zone I

FIG. 9.



Zone III

FIG. 10.



Zone V

FIG. 11.

33. Finding focus.

The principle underlying the zonal test is shown in fig. 12.

Here ACD and BCE are rays of light reflected from the speculum AB and coming to a focus at C. Now it is evident that if the shutter be at F, and be moved into the cone of rays, it will

cut off B C E first, which means that to the eye placed behind the shutter the portion B of the mirror will appear to darken first. If now the shutter be moved back to H, the ray A C D will be the first to be cut off, and A will darken first. At A the shutter will cut into the focus C, and both sides of the speculum will darken simultaneously and evenly. When the tones on both sides exactly match, and when the two sides darken evenly and simultaneously, we know that we are exactly in the focus. A little practice is required, but the art is soon learnt, and the focal point can then

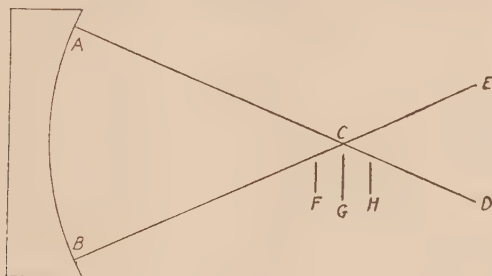


FIG. 12.

be fixed to $\cdot 01$ inch, if not less. For the purpose of ascertaining the focal point of the outside zone both the wood-screw H and the set-screw S (figs. 2, 3, 4, 6) will be needed. (See § 18.) With the wood-screw H set the index to the zero of the scale, and then with the set-screw S find the focus of the outside zone. This should be found with the greatest possible care and precision, as it forms the starting-point for the measures of all the other zones. After it has been ascertained the set-screw S must not be touched again, the foci of all the other zones being found with the wood-screw H, and their measures read off on the scale.

34. *Precision required in zonal measurement.*

It is these measures of the zonal foci that constitute the zonal test. They should be made several times, and the mean of the readings taken. Especially should this be done with the zones nearer the centre, whose foci, as mentioned before, are less easy to fix. It should be stated that in actual practice the last zone is always omitted, being merely a round hole in the centre.

35. *Eyepiece objectionable.*

By some workers an eyepiece has been employed instead of the shutter to fix the foci. This is open to serious objections on account of the well-known accommodating power of the eye, which automatically adapts itself to images slightly out of focus and so gives false readings for the focal points.

36. *Turned-down edge.*

One of the faults most commonly found in mirrors is that known as "turned-down edge." It causes the extreme margin of the mirror to have a much longer focus, and is rendered plainly apparent when measuring the outside zone, and accordingly the present seems a fitting opportunity to speak of it. In the case of a perfect mirror, when the shutter by means of the set-screw P is caused to cut into the cone of rays exactly at the focus of the outside zone, the two segments of the zone should darken evenly and simultaneously. If any motion of the shadow is at all perceptible, that motion should be a simultaneous advance inwards from the rim of the mirror on either side, that is, the shadows on the two segments should look as if they were starting from the rim of the mirror on each side, and were about to meet at the centre of the mirror. When the edge is turned down they cannot be persuaded to act in this way; and if the shadow be caused to come on the mean radius of the zone simultaneously on both sides, it will be found, on observing the progress of the shadow across the zone as a whole, that it advances on the two sides in the same direction, viz. from left to right.

The foregoing is certainly the appearance when the "turn-down" is prominent, or when it extends inwards to anywhere near the width of the outside zone. When, however, the turn-down is confined within narrower limits, such as less than $\cdot 25$ inch, the appearance may not be so conspicuous in this respect, but it betrays itself in another way. As the screw P is turned further, the left rim of the mirror will become quite dark, while there is still some light left on the other parts of the zone, and as the shutter is carried still further into the cone of light, the two portions of zone will gradually darken all over except the right-hand rim of the mirror, which will still remain light, and will not darken till the screw has another half turn or more given to it.

When there is no turned-down part the phenomena presented are the same as those of the other zones, viz. that the zone as a whole darkens fairly evenly all over, and when it has just darkened entirely, there remains the usual diffraction phenomenon, viz. a thread-like line of light lingering all round the borders.

37. *Shadow test.*

We now turn to the second part of the test, generally known as the "shadow" test. The essential feature here is that no diaphragm is employed, the whole surface of the mirror being viewed at once. Tested by this means the mirror immediately reveals any irregularity of curve or want of symmetry that may exist on any portion of it.

Supposing for a moment that the mirror is a spherical one, in which case the focal point is the same for all portions of the surface, it is evident that if the shutter be caused to cut off the rays exactly

at this focal point, the result will be that the mirror will appear to darken evenly and simultaneously all over. This is the characteristic of the spherical mirror, and of the spherical mirror only. In the sense of one portion of the surface being more shaded than another the spherical mirror yields no "shadow." As the shutter advances the surface loses brightness, then turns grey, and lastly quite dark.

But when, as in the case of the parabola, the radius of curvature decreases from the circumference towards the centre, we encounter quite another set of phenomena. Let us study the following figure.

38. *The parabolic shadow.*

Here the speculum is divided into six zones, of which the focal points are designated by the letters of the rays whose crossing forms the focus at that point. Thus Dd is the focal point of the rays D and d , that is of the fourth zone, reckoning from the outermost zone, and so on. Now let the shutter cut into the cone of rays at the focal point of the outermost zone Aa , and note carefully what happens. The first ray to be cut off is d . That is to say, a shadow will begin to appear at d on the face of the mirror, midway between the centre and the right-hand rim. Now move the shutter a trifle farther. The next ray that it cuts off is e . This means that the shadow which began at d has spread till it has reached e . Very rapidly the shutter cuts into the ray c , this being accompanied by a correspondingly quick growth of the shadow on its right side, where it has spread to c . It will easily be seen that this process continues till it engulfs b and f , that is to say, that it gradually overspreads almost the whole of the right-hand side of the mirror. At this point, as the shutter draws near to the focal point Aa a very slight shading comes on at the left-hand edge of the mirror, and before it has assumed any noticeable depth of tone it reaches the point A , and simultaneously with this a similar shading overspreads a . When this point is reached the shutter has arrived exactly at the focal point Aa . It will be found that now the whole of the right half is more or less in shade, and, further, that a shading has spread round the outermost zone of the left half, leaving an elliptically shaped island of light on the left half. If the figure be studied, it will be found that any further advance of the shutter will result in a contraction of the light area. This proceeds in an order which is the exact counterpart of the manner in which the shadow originally grew on the right half, that is, the shadow spreads inwards from B and F through C and E , the last spot to darken being midway between the centre of the mirror and its left rim, this being effected by the cutting off of the ray D by the fully advanced shutter. The last glimmer of light on the middle of the left half thus corresponds with the first appearance of shadow on its right half, and all the other intermediate appearances are similarly matched, or rather, contrasted, on the two halves of the mirror.

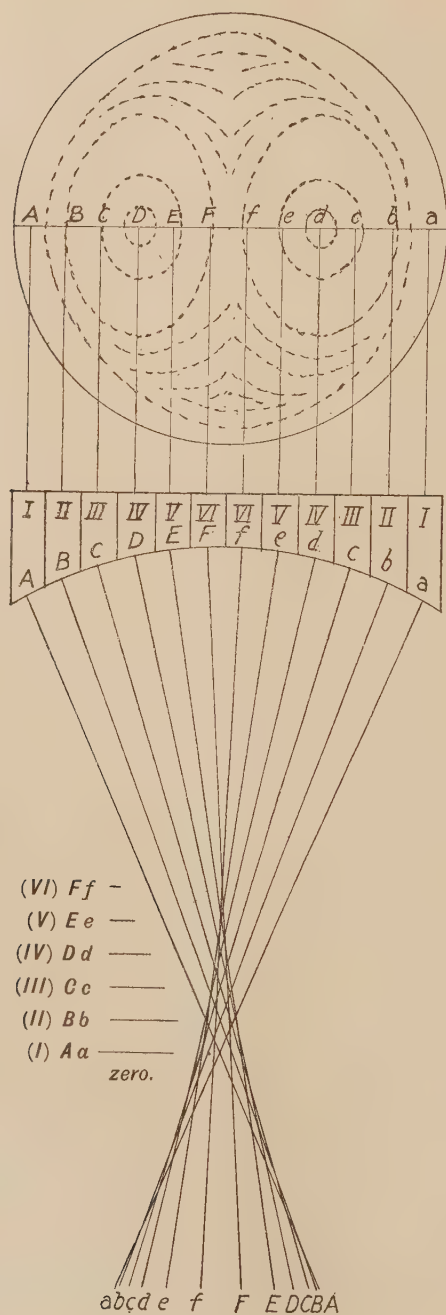


FIG. 13.

39. *The spheroidal shadow.*

A mirror in which the curvature increases in passing from the centre towards the rim, termed by mirror makers "spheroidal," when examined with the shutter at the focus of the outermost zone, will exhibit phenomena the exact reverse of those just described. The first appearance of shadow is on the left, and the last glimmer of light on the right. It is thus instantly recognised and condemned. (See section 21 *ad fin.*)

40. *The shaded drawings.*

Through the kindness of Rev. Charles Walters of Forthampton, Gloucestershire, my original tutor in the fascinating pursuit of grinding, figuring, and testing mirrors, I am enabled to give the accompanying set of admirable shaded drawings, illustrating some typical appearances of mirrors viewed under the shadow test from the focal point of their outside zone.

41. *Parabola of long focus.*

The first of these (Plate 14, fig. 14) exhibits the appearance of a parabolic mirror whose focal length is about ten times its diameter. The shutter is just touching the crossing point of the rays of the zero zone. This is indicated by the fact that the shadow has crept nearly but not quite over the right half, and is just beginning to show itself around the whole edge.

42. *Delicacy of the shadow.*

The one point to be noted above all others is the exceeding delicacy of the shadow. This is beautifully rendered in the drawing. It is impossible to insist on this feature too forcibly. In spite of what Wassell and Blacklock have written, it seems almost hopeless to impress this on the mind of the average worker, who seems to think that because "the shadows come on right," he has therefore got a parabolic mirror; the inevitable result of this fallacy being that the tone of shading is in reality far too deep, and the mirror markedly, often profoundly, over-corrected.

43. *Zonal test absolutely essential.*

In most instances the crux of the question lies in the temptation to shirk the trouble of making a proper and effective zonal measuring apparatus, of cutting out diaphragms, and of calculating zonal aberration, the result being that zonal measures are not properly taken, or even not taken at all. When it is considered that the depth of the shadow varies not only with that of the curve, but also with the different ratios of focal length to aperture, it may easily be conceived that not merely is the tyro likely to be at sea who presumes to trust to the shadow, to the exclusion of the zonal test, but that this fate may befall, and, as I happen to know, does

befall, the most experienced workers. Let me therefore once more impress upon all who would produce any approach to a perfect mirror the absolute necessity of employing the zonal test with the utmost rigidity and exactitude in conjunction with the shadow test.

44. *Form of apparent solid.*

To see the drawing to the best advantage, it should be viewed from the proper distance, *i.e.* from a point about 20 of its diameters away from it, and with the light coming from the left. The appearance which it then presents is that of a surface of the form shown in elevation in figure 14a.

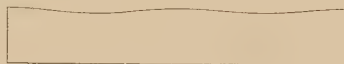


FIG. 14a.



FIG. 14b.



FIG. 14c.



FIG. 14d.



FIG. 16a.

The essential points in this apparent figure are the central mound rising gently and evenly from a circular shallow depression whose lowest portion lies midway between the rim and the centre.

45. *Some faulty forms.*

The next three figures show some of the commonest forms of erratic figure. Of these 14b, in which the mound is too high and too sharp at the summit, and in which the gully is too narrow, too

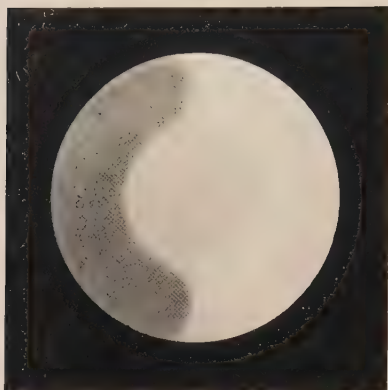


Fig. 20.

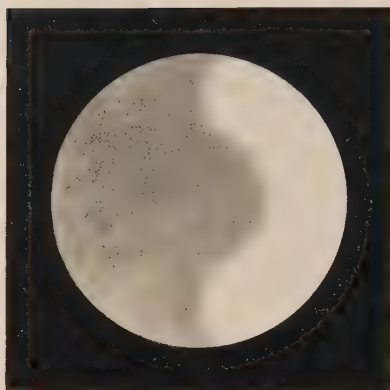


Fig. 21.



Fig. 22.

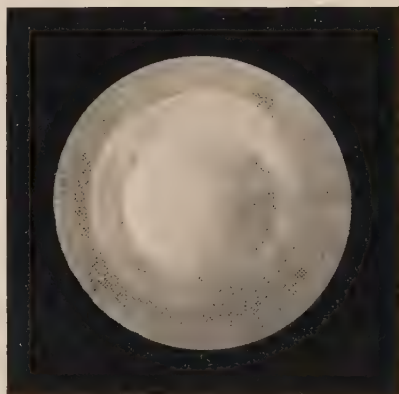


Fig. 23.



Fig. 24.

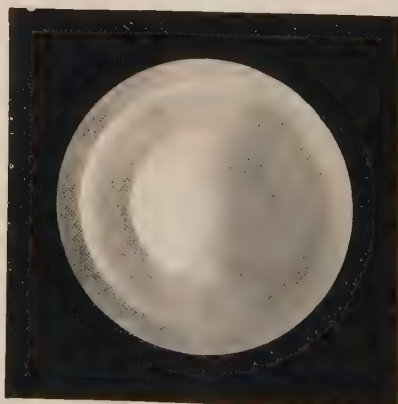


Fig. 25.



Fig. 14.



Fig. 15.



Fig. 16.

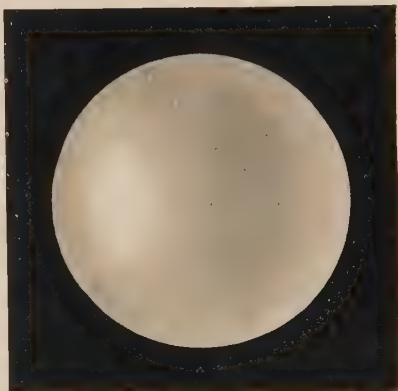


Fig. 17.

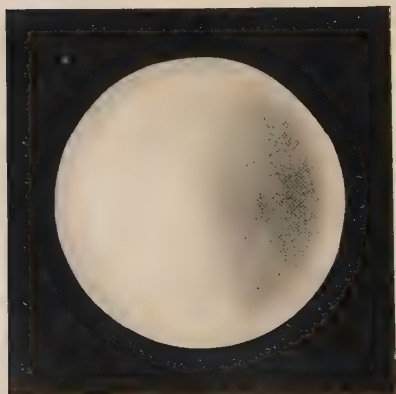


Fig. 18.



Fig. 19.

deep, and too near the rim, is characteristic of a mirror violently over-corrected. Fig. 14c shows a hyperbola with an apparent central hole, and fig. 14d exhibits a mound fortified by a rampart and a fosse.

46. *Some normal shadows.*

The next three drawings (figs. 15, 16, 17) constitute a set by themselves, being illustrations of a mirror whose focal length is about seven times its aperture. They show three consecutive appearances as the shutter advances. Fig. 15 illustrates the appearance of the shadow just after its commencement, midway between the centre and right edge of the mirror. Fig. 16 shows it as the shutter is just touching the focal point of the outside zone, and is strictly comparable in this respect with fig. 14. But note carefully that the shading is deeper, and that the shape of the apparent solid is more like that represented in fig. 16a than that in 14a, that is, the central mound and circular valley are more pronounced. In spite of the fact that the shadows are deeper, the mirror is truly parabolic, thus illustrating the fallacy of attempting to distinguish the parabola solely by the depth of its shadow.

Fig. 17 gives a view of the same mirror with the shutter advanced to nearly its full extent, and is interesting as being illustrative of the remarks in section 38, *ad fin.* Taking it in connection with fig. 15, it is the exact counterpart of the latter, showing, as it does, how and where the light last disappears on the left side, while the other shows how the shadow first appears on the right.

47. *"Over-corrected" shadow.*

The drawing fig. 18 is a perfect representation of a typical over-corrected mirror. Reverting for a moment to fig. 14b it will at once be seen that this and that tally with each other in every particular,—the narrow gully too near the edge, the central mound too wide, too high, and too sharp at its summit.

48. *Shadow seen from focus of centre.*

Figs. 20 and 21 are most instructive. In what has hitherto been discussed, the mirror has been examined from the focus of the outer zone as view-point. But there is another point of view from which also it should invariably be scrutinised with closest attention, viz. from the theoretical focus of the centre, or perhaps better, very slightly within it. These two figures give the appearance of a mirror under this particular test. A glance at fig. 13 will show that the shutter, in cutting into the cone of rays at the point Ff , shuts them all off in regular order, no crossings having yet occurred. From this flow two consequences. First, the shadow proceeds across the whole mirror in the same direction as the shutter, *i.e.* from left to right. Second, the apparent shape or

form of the mirror is its real form, *i.e.* a mound or hillock appears as such, and not as a pit; a ditch appears as a ditch, and not as a rampart. The proper mode of progress of the shadow with the shutter in this position may be compared with the waxing moon in the reverse order. As the shutter's edge impinges on the cone of rays, the full-moon appearance of the mirror gives way to that of a gibbous moon 13 or 14 days old. Midway the appearance is that of the moon at first quarter, that is, the edge of the shadow should be straight, and should coincide with the vertical diameter of the mirror. Then comes the appearance of the crescent, and so on till the mirror is dark. Tested in this way, not only are any irregularities of curvature seen as such, but their presence is corroborated by a corresponding irregularity in the terminator. This test is a very searching and instructive one, and should never be omitted. Taken in conjunction with the test at the focal point of the outside zone, no room is left for any doubt as to the regularity, or want of it, in the curve of the mirror.

49. *Other focal view-points not necessary.*

The focal point of the zone whose radius is half that of the rim has sometimes been recommended as the best point from which to observe the shadows and their behaviour, but it is not necessary to do this.

50. *A faulty mirror.*

In figs. 20 and 21 we see a faulty mirror under the shadow test at the focus of its central portion. In fig. 20 the shadow has all but reached the vertical diameter, indicating that the shutter has cut almost into the centre of the cone of rays. But the shadow has invaded only the outer half of the surface with any degree of intensity. Hence we are within, and by the comparative definition and crispness of the edge of the shadow are well within, the focus or foci of these zones. But in addition to this, note that the terminator is not straight; it falls away from the vertical diameter both on the outside and on the inside limits of the zone in shadow. This indicates that the outside edge is turned down, and that there is a gradual slant downwards from the shaded portion to the still unshaded central portion. Looking at the next drawing (fig. 21), we find, as indicated by the outer portion of the mirror where the shadow has advanced the merest trifle further to the right, owing to the shutter having been advanced a correspondingly minute distance, that the shadow on the central portion has suddenly taken a huge leap forward, and that, whereas it previously lagged behind the other shadow, it is now far ahead of it—in fact, that the light from the central portions has been quite cut off. These appearances show that the shutter is well within the focus of the outer portions, and that it is just at, or exceedingly close within, the focus of the central portion, which last is of *spherical* figure. The mirror is, in fact, compounded

of at least two curves—an outer long-focussed and possibly “spheroidal” portion, surrounding an inner shorter-focussed and spherical area.

51. *Flexure.*

Turning now to fig. 19, we come to the question of flexure, the slightest trace of which is vividly revealed by the shadow test. Here is a mirror which, left to itself, is of perfect paraboloidal figure, but which, for the purposes of this illustration, has been temporarily flexured across one diameter, thus furnishing an instance of “simple” flexure. It is at once evident that the shadow is wholly abnormal, the top and bottom being quite out of correspondence with each other. The top is in decided shadow, the bottom being in bright light, the explanation of the phenomenon being that there are two radii of curvature, that from the portions of the mirror in and adjacent to one diameter being longer than the others.

52. *Precautions in testing for flexure.*

Flexure is only revealed by this test when it is along a diameter neither vertical nor horizontal; hence in testing for flexure it is always essential that the mirror (supposing no signs of flexure to have been seen at the first trial) should be tested at least once more, being rotated 45° or thereabouts previous to the second test. To make matters quite certain, it is strongly advisable to test more than twice, in positions removed each time about half a right angle from each other. In this way, too, cases of compound flexure, that is, of mirrors with two or more bends of any length and position with regard to each other are sure to be detected. The depth of shading in the drawing has been purposely exaggerated a little to render its form and position plain. In practice, the slightest difference between the upper and lower half, either in the form or depth of the shadow, should be carefully noted, since a flexured speculum will be incapable of the highest work.

53. *Cell may be at fault.*

On the other hand, if the mirror being tested should happen to be at the time mounted in its cell, the flexure may be, and more often is, due entirely to some pressure from the cell, which some slight adjustment may put right in a moment.

54. *Some actual testings.*

The next four drawings (figs. 22, 23, 24, 25) represent four mirrors that Mr. Walters and I tested together in 1905 by the kindness of Professor Turner, at the University Observatory at Oxford. It had long been my desire to examine some works of the older masters in speculum-making, and I am much indebted to the Professor for allowing me access to them.

Fig. 22 represents a mirror by With of 15 inches diameter and 125 inches focal length. The zonal measures were—

Zone.	<i>r.</i>	<i>a.</i>	Observed.
I.	6·875	·000	·000
II.	5·625	·032	·070
III.	4·375	·062	·113
IV.	3·125	·099	·110
V.	1·875	·163	·168

Viewed by the shadow test, the chief noticeable features were a flat edge and a small central hole.

Fig. 23 represents a 10-inch mirror by With of 80·5 inches focal length. The following are the zonal measures :—

Zone.	<i>r.</i>	<i>a.</i>	Observed.
I.	4·583	·000	·000
II.	3·75	·043	·046
III.	2·916	·078	·062
IV.	2·08	·104	·088
V.	1·25	·122	·100

A comparison of the computed with the observed aberrations shows the mirror to be somewhat under-corrected. This is in full accord with the usual custom of the older workers. It was an axiom with them that under-correction was an infinitely less evil than over-correction: whether or not they were right I am not prepared to say.

The drawing shows that there is a turn-down edge, and that the parabolisation, instead of proceeding in one continuous curve, goes by steps. There are two distinct “rings” on the surface, with a gully (see the measure of zone II.) between them.

The next mirror (fig. 24) is a 13-inch of 120 inches focal length by Warren De la Rue, and is the best of the Oxford mirrors that were tested.

Experience having taught us that in these older mirrors a turn-down edge was to be expected, it was resolved to treat this mirror in the first instance as a 12-inch, that is, to take the zone with the mean radius 5·5-inch as the zero zone, and to regard the real outside zone of ·5-inch breadth as an extra zone, whose aberration would then have to be measured backwards from the zero point. The reason for this mode of procedure lies in the fact that, as previously explained, when the edge is turned down, it is difficult to fix the focal point of the outside zone with exactitude, owing to the want of evenness in the shading.

Zone.	<i>r.</i>	<i>a.</i>	Observed.
Extra.	6·25	—·036	—·05
I.	5·5	·000	·000
II.	4·5	·042	·056
III.	3·5	·075	·05
IV.	2·5	·1	·075
V.	1·5	·117	·1

Viewed by the shadow test, the mirror showed a surface more free from rings and other inequalities than the other three mirrors. Besides the turned-down edge already mentioned, there is a ring corresponding with the over-correction of zone II. Then comes a portion (zones II. and III.) which is practically spherical, after which comes the interior, which is fairly parabolic.

The last drawing (fig. 25) illustrates a 13-inch mirror by Steinheil of 118.75 inches focus. The same diaphragms and aberrations were used for this mirror as for the preceding one, the two being of the same diameter, and of closely approximate focal length.

Zone.	<i>r.</i>	<i>a.</i>	Observed.
Extra.	6.25	— .036	
I.	5.5	.000	.000
II.	4.5	.042	.05
III.	3.5	.075	.09
IV.	2.5	.1	.19
V.	1.5	.117	.195

Here we have a distinctly over-corrected but irregular curve. Zones II. and III. are not so very far from the correct depth, but then comes the serious descent of zone IV., succeeded by the comparatively spherical portion near the centre, as indicated by the small difference in the measures of zones IV. and V. Of the extra zone I have no measure, which probably means that it ran beyond the scale. The turn-down is undoubtedly deep. The mirror is, on the whole, rather a poor one.

It only remains to set down three or four concluding notes of a general nature.

55. *Concluding remarks.—Draughts.*

Beside the tremors due to any want of stability in the floor, those arising from draught or layers of air of different temperature should be carefully avoided. In this connection, Draper has an instructive woodcut showing the vapours resulting from the hand held between the observer and the mirror.

56. *Equality of temperature.*

For final testings, it is always best to mount the mirror on its support, and leave all quiet for an hour or so beforehand to ensure thorough equality of temperature. With preliminary testings carried out during the process of figuring, this cannot invariably be done, as it would entail too much loss of time.

57. *Eyepiece useless in shadow test.*

A warning was uttered against the use of an eyepiece for measuring zonal foci. Its employment is equally unsatisfactory in connection with the shadow test. I do not mean that it is invari-

ably and absolutely useless, but that, if used at all, it should be merely in a secondary and subsidiary manner. Occasionally, doubtless, something may be learnt from the "in and out of focus" test; but, as a means of judging of the truth and regularity of the curve, still more of showing the exact position and extent of any irregularity, its employment as a substitute for the shadow test appears to me a distinctly backward step.

58. General advantages.

In conclusion, I will only add that the method which I have described is one possessing every qualification for efficiency. It is simple both in theory and in practice. It is, unlike testing by means of a star, available at any time of the day or night, and is quite independent of the weather. More important still is the fact that it is conducted without the intervention of intermediate lenses, mirrors, or other possible sources of error, the light simply issuing from the pin-hole, and thence falling direct on the mirror, by which it is reflected immediately to the shutter-edge, where it tells its own unvarnished tale.

TABLE OF ABERRATIONS.

This table is calculated by the formula

$$a = \frac{r^2}{R},$$

where a = the longitudinal aberration.

r = radius of the given zone on the face of the mirror.

R = the radius of curvature of the mirror at its vertex.

The limits of the table are determined for (1) the central portion of the mirror, by the fact that quantities less than about .005 inch are practically negligible, and for (2) the circumference, by the consideration that mirrors of focal length less than thrice their diameter are seldom, if ever, wanted.

Within these limits the table is available for mirrors to one of 20 inches diameter and of eight times its diameter in focal length; while for those of smaller diameter, its range in this respect is considerably greater.

In any case in which it may be thought desirable to find the aberration as given by the complete formula,

$$\begin{aligned} a &= \frac{r^2}{R} + \frac{r^4}{2R^3} \\ &= \frac{r^2}{R} + \left(\frac{r^2}{R}\right)^2 \times \frac{1}{2R}; \end{aligned}$$

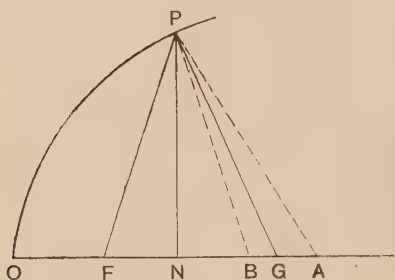
then square the quantity given by the table, divide the square by $2R$, and add the quotient to the tabular quantity.

It only remains to state that quantities throughout the table are given in inches.

R.	5	'75	1'0	1'25	1'5	1'75	2'25	2'5	2'75	3'25	3'5	3'75	4'5	5'5	6'5	7'5	8'5	9'5
24	'010	'023	'042	'065	'094	'128												
30	'008	'019	'033	'052	'075	'102												
36	'007	'016	'028	'043	'062	'085	'169											
42	'006	'013	'024	'037	'053	'073	'120	'149	'210									
48	'005	'012	'020	'032	'047	'064	'105	'130	'157	'251								
54	'005	'010	'018	'029	'042	'057	'094	'116	'140	'196	'255							
60	'004	'009	'017	'026	'037	'051	'084	'104	'126	'176	'204	'292						
70	'003	'008	'014	'022	'032	'044	'072	'089	'108	'151	'177	'201	'234	'432				
80	'003	'007	'012	'019	'028	'038	'063	'078	'094	'132	'153	'176	'253	'378	'528			
90	'003	'006	'011	'017	'025	'034	'056	'069	'084	'117	'136	'156	'225	'336	'469	'625		
100	'002	'006	'010	'016	'022	'030	'051	'062	'076	'106	'122	'141	'202	'302	'422	'562		
110		'005	'009	'014	'020	'028	'046	'057	'069	'096	'111	'128	'184	'275	'384	'511	'657	'752
120		'005	'008	'013	'019	'025	'042	'052	'063	'088	'102	'117	'169	'252	'352	'469	'602	'694
130		'004	'008	'012	'017	'023	'039	'048	'058	'081	'094	'108	'156	'233	'325	'433	'556	'645
140		'004	'007	'011	'016	'022	'036	'045	'054	'075	'088	'100	'145	'216	'302	'402	'516	'602
150		'004	'007	'010	'015	'020	'034	'042	'050	'070	'082	'094	'135	'202	'282	'375	'482	'564
160			'006	'010	'014	'019	'032	'039	'047	'066	'076	'088	'126	'189	'264	'351	'451	'531
170			'006	'009	'013	'018	'030	'037	'044	'062	'072	'083	'119	'178	'249	'331	'425	'501
180			'005	'009	'012	'017	'028	'035	'042	'059	'068	'078	'112	'168	'235	'312	'401	'501
190			'005	'008	'012	'016	'027	'033	'040	'055	'064	'074	'106	'159	'222	'296	'380	'475
200			'005	'008	'011	'015	'025	'031	'038	'053	'061	'070	'101	'151	'211	'281	'361	'451
210			'007	'007	'011	'014	'024	'030	'036	'050	'058	'067	'096	'144	'201	'268	'344	'430
220			'007	'007	'010	'014	'023	'028	'034	'048	'056	'064	'092	'137	'192	'256	'328	'410
230			'007	'007	'010	'013	'022	'027	'033	'046	'053	'061	'088	'131	'184	'245	'314	'392
240			'006	'006	'009	'013	'021	'026	'031	'044	'051	'059	'084	'126	'176	'234	'301	'376
250			'006	'006	'009	'012	'020	'025	'030	'042	'049	'056	'081	'121	'169	'225	'289	'361
260			'009	'009	'009	'012	'019	'024	'029	'041	'047	'054	'078	'116	'162	'216	'278	'347
280			'008	'011	'008	'011	'018	'022	'027	'038	'044	'050	'072	'108	'151	'201	'258	'322
300			'007	'010	'007	'010	'017	'020	'025	'035	'041	'047	'067	'100	'141	'187	'241	'300
320			'007	'009	'007	'009	'016	'019	'024	'033	'038	'044	'063	'094	'132	'178	'226	'282

APPENDIX.

Proof of the Formula for the Longitudinal Aberration of a Paraboloid at the Centre of Curvature. By H. C. Plummer, M.A.



P is a point on the mirror.

F is the focus, $OF = f$.

PN, the ordinate $= y$.

$$ON = x = \frac{y^2}{4f}.$$

PG is the normal.

$$\angle OFP = v, \quad \angle OGP = \frac{1}{2}v,$$

$$\angle ONP = \phi, \quad \angle OAP = \theta.$$

AP, PB are the incident and reflected rays.

$$OA = h, \quad OB = k.$$

$$\therefore \theta + \phi = v, \quad \tan \frac{1}{2}v = \frac{y}{2f}.$$

$$\tan \theta = \frac{y}{h - x} = \frac{4fy}{4fh - y^2}, \quad \tan \phi = \frac{y}{k - x} = \frac{4fy}{4fk - y^2}$$

$$\therefore \tan(\theta + \phi) = \frac{8fy(2fh + 2fk - y^2)}{(4fh - y^2)(4fk - y^2) - 16f^2y^2} = \tan v = \frac{4fy}{4f^2 - y^2}$$

$$\therefore 4f(4f^2 - y^2)(h + k) - 2y^2(4f^2 - y^2) = 16f^2hk - 4fy^2(h + k) + y^2(y^2 - 16f^2)$$

$$\therefore 16f^2hk - 16f^3(h + k) - y^2(y^2 + 8f^2) = 0.$$

Let $2y = D =$ diameter of zone and $2f = R =$ radius of curvature at O. Put $h = p + R$, $k = q + R$.

[p and q are distances of source and image from centre of curvature measured *away* from mirror.]

Last equation becomes

$$4R^2pq + 2R^3(p + q) - \frac{D^2}{4} \left(\frac{D^2}{4} + 2R^2 \right) = 0.$$

If source is kept at centre of curvature at O, $p = 0$, and

$$q = \frac{D^2}{8R^3} \left(\frac{D^2}{4} + 2R^2 \right) = \frac{D^2}{4R} + \frac{D^4}{32R^3}.$$

This is the required aberration. The formula is exact. The second term is generally negligible in comparison with the first.

Another method is to move the source so as to make the image coincide with it, in which case $p = q$. Then

$$R^2(2q + R)^2 = R^4 + \frac{1}{4} \left(\frac{D^2}{4} + 2R^2 \right) = \left(R^2 + \frac{D^2}{4} \right)^2, \text{ giving } q = \frac{D^2}{8R}.$$

This is Draper's method and formula.

The Recent Pendulum Observations in India.

By Major G. P. Lenox-Conyngham, R.E.

At the beginning of 1904 a new series of pendulum observations for the determination of the force of gravity was begun in India.

The apparatus was of the von Sterneck pattern, and included four half-seconds pendulums. It was made in Vienna, but was fitted in Potsdam with the auxiliaries required for the determination of the effect of the flexure of the stand by Professor Schumann's method.

Five tours have been made with the apparatus since its arrival in India, but it is not my intention to give in detail the results that have been obtained, as they are in course of being brought out in one of the publications of the Survey of India. I merely propose at present to touch on points of special interest, more particularly as they bear on the analysis of the deflections of the plumb-line from the normal to the Earth's surface, which was given by Colonel Burrard in the *Monthly Notices* of January 1902, and subsequently, in greater detail, in Professional Paper No. 5 of the Survey of India.

It may be well to recapitulate shortly the results of that analysis.

Colonel Burrard showed that the deflections revealed by the latitude observations arrange themselves in zones which appear to be parallel to the general trend of the Himalayan Ranges.

There is first, at the foot of the hills, a zone of large northerly deflections, which decrease rapidly as the distance from the hills increases. Secondly, there is a zone of southerly deflections; and thirdly, a second zone of considerable northerly deflection.

Up to the time of the publication of Colonel Burrard's analysis it had been the custom to ascribe the rapid falling off of the attraction of the Himalayas on the plumb-line to "mountain compensation"; that is to say, to the theory that deficiencies of density underlying mountains compensate the apparent excess of mass, so that the amount of matter in the crust of the Earth is the same, or nearly the same, in mountainous regions as elsewhere.

Other anomalies in the directions assumed by the plumb-line were attributed to purely local variations in the density of the surface strata.

Colonel Burrard showed that the theory of compensation was incapable of explaining both the large deflections at the foot of the hills and the rapid falling off that occurs at moderate distances from them; and further, by exhibiting the way in which the deflections can be classified, he made it clear that their causes were not purely local in character.

To account for the observed phenomena, he showed that it is necessary to suppose that the Himalayas are compensated to some extent, and that there exists "an invisible chain of excessive density" running parallel to the Himalayas, right across India

from the Bay of Bengal to the Punjab. I may mention that all the latitude observations which have been made in those regions since he put forward this idea go to corroborate it.

A general gravimetric survey of India is the object with which the pendulum observations have been begun, but in planning some of the first programmes of observation it was thought desirable to test, as far as was convenient, Colonel Burrard's deduction as to the distribution of mass in the hills and in the plains below them; that is to say, to find out by means of the pendulum whether the force of gravity in the Himalayas is less than it would be if the material composing them were of average density, and to see whether at points situated over the supposed hidden chain of excessive density the force of gravity is correspondingly high.

The mean surface density of the Earth may be taken to be 2·8, and the mean density of the Earth as a whole 5·6, or twice the surface density.

To determine whether the observed force of gravity is in excess or defect of the normal, we must know what is the normal force for the station of observation.

To find the normal force in a given latitude, at sea-level, Helmert's formula has been used, namely:—

$$\gamma_0 = 978\cdot00(1 + 0\cdot005310 \sin^2 \phi)$$

in centimetres per second per second.

This quantity requires in general three corrections to give the value at the station of observation:

First, the correction on account of the height above sea-level,

$$-\gamma_0 \frac{2h}{R},$$

where h is the height of station above sea-level, and R is the mean radius of the Earth.

Secondly, the correction on account of the attraction of the mass between sea-level and the station. If the station is situated on an extensive level plateau, the material underlying it having a density of 2·8, this correction is

$$+\gamma_0 \frac{3}{4} \frac{h}{R}.$$

If the surroundings of the station do not resemble a level plateau, a correction is required to allow for the difference. This is called the orographical correction; it has to be separately computed for each station, and depends on the topography of the region.

In practice, it is more convenient to apply these corrections to the observed force of gravity g , and to compare this corrected value, called g''_0 , with the value of γ_0 given by the formula.

We thus obtain for every station the quantity $(g''_0 - \gamma_0)$. If this is positive it denotes that gravity is in excess of the normal; and if negative, that it is in defect.

To give a material interpretation to the quantity $(g''_0 - \gamma_0)$ we may suppose that an excess in g is caused by an excess of matter in the form of a horizontal disc of indefinitely great radius underlying the station, when the thickness of the disc will be proportional to the observed excess in g .

The attraction of a disc of infinite radius on any point in its axis is

$$= 2\pi h\delta G,$$

where h is its thickness, δ its density, and G the gravitational constant.

The attraction of the Earth on a point on its surface

$$= \frac{4}{3}\pi R\Delta G,$$

where R is the mean radius and Δ the mean density.

Hence, calling the attraction of the disc A , and that of the Earth g , we have

$$\frac{A}{g} = \frac{3}{2} \frac{h}{R} \frac{\delta}{\Delta}.$$

Putting $\frac{\delta}{\Delta} = \frac{1}{2}$, $R = 21000000$ ft., and $g = 978$ cm.

$$A = h \times 0.000035,$$

A being expressed in centimetres, and h in feet.

Therefore the thickness of a disc of density 2.8, which will give a value of 0.001 cm. to A is 28.6 feet; in other words, the presence of an excess of matter equivalent to a disc 28.6 feet thick will account for a value of +0.001 cm. in $(g''_0 - \gamma_0)$.

The first set of observations carried out with a view to testing Colonel Burrard's theory began at the station of Chatra, latitude $24^\circ 13'$, longitude $88^\circ 23'$, height above sea-level, 64 feet; and, running approximately up the meridian, ended at Sandakphu, latitude $27^\circ 6'$, longitude $88^\circ 0'$, height above sea-level, 11,766 feet.

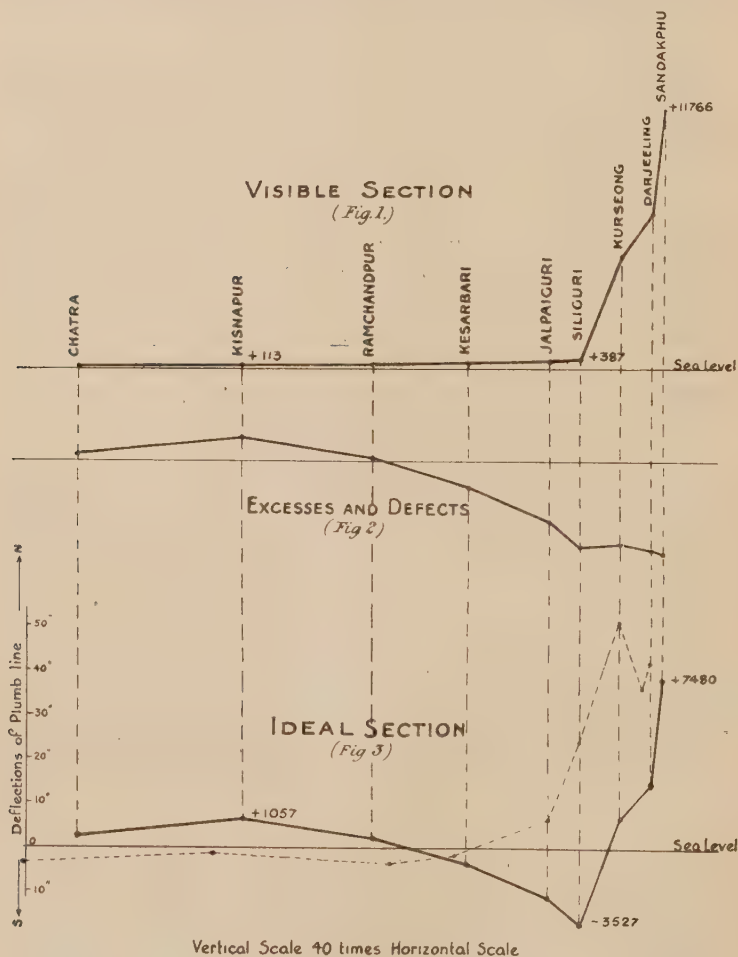
The position of the stations, the values of $(g''_0 - \gamma_0)$, and the thicknesses of the discs of excess or defect are given in Table I.

TABLE I.

Station.	Latitude.	Longitude.	Height.	$g''_0 - \gamma_0$.	Thickness of Disc.
			feet.	cm.	feet.
Chatra. . .	$24^\circ 13'$	$88^\circ 23'$	64	+0.009	+ 257
Kisnapur . .	$25^\circ 2'$	$88^\circ 28'$	113	+0.033	+ 944
Ramchandpur .	$25^\circ 41'$	$88^\circ 33'$	132	+0.001	+ 29
Kesarbari . .	$26^\circ 8'$	$88^\circ 31'$	204	-0.043	- 1230
Jalpaiguri . .	$26^\circ 31'$	$88^\circ 44'$	268	-0.096	- 2746
Siliguri . . .	$26^\circ 42'$	$88^\circ 25'$	387	-0.137	- 3914
Kurseong . .	$26^\circ 53'$	$88^\circ 17'$	4,913	-0.130	- 3718
Darjeeling . .	$27^\circ 3'$	$88^\circ 16'$	6,966	-0.143	- 4090
Sandakphu . .	$27^\circ 6'$	$88^\circ 0'$	11,766	-0.150	- 4286

Three diagrams have been drawn to show the meaning of these figures.

Fig. 1 is a section of the country as we see it, but with the vertical scale exaggerated—it is 40 times the horizontal scale. The plain rises very slowly and quite evenly until the foot of the



hills is reached at a point just north of Siliguri; thence there is a steep climb all the way to Sandakphu.

In fig. 2 the ordinates represent, on same scale as in fig. 1, the thicknesses of the discs of excess or defect; and in fig. 3 the ordinates are the algebraic sums of those in figs. 1 and 2; this diagram therefore gives an ideal section showing the form the country would

take if the density were made homogeneous by compression or expansion as the case might be.

Beginning at the northern end, we see that there is a deficiency of density underlying the Himalayas, but that it does not amount to total compensation.

In the case of Sandakphu the height must be reduced by about one-third to give the true efficacy of the hill as an attracting mass. In the other stations the proportion of reduction is higher, for the deficiencies are nearly the same, and the heights less. Under Siliguri, which is in flat ground only 387 feet above sea-level, the deficiency is almost as great as at Sandakphu. So that in the ideal section this station is 3527 feet below sea-level. In other words, if there were no variations of density, there would be a great ditch at the foot of the hills.

Moving south from Siliguri, the deficiency gradually becomes less, and at Ramchandpur, about 80 miles from the hills, it gives place to a small excess; this increases, and attains a maximum at Kisanpur some 50 miles further south.

In fig. 3 the dotted line shows the variations in the deflections of the plumb-line which have been revealed by the latitude observations. When the curve is above the datum the deflections are to the north, and when below it to the south.

It is easy, with this ideal section before one, to see why there should be southerly deflections between Ramchandpur and Kesarbari, for the great dip to the immediate north largely counteracts the effect of the more distant Himalayas while the Kisanpur hill is exerting its full influence.

The very rapid increase in the northerly deflections between Jalpaiguri and Kurseong is also quite intelligible: at Jalpaiguri there are about equal amounts of defect north and south of the station, and the northerly deflection measures the preponderance of the Himalayan attraction over that of the Kisanpur hill: at Siliguri the greater part of the "ditch" is to the south, and the Himalayas more immediately to the north: and at Kurseong the negative attraction of the ditch and the positive one of the Himalayas are acting wholly in concert to produce a northerly deflection of the plumb-line—the greatest that has been observed in India.

An extension both of the latitude and of the pendulum observations to the south of Chatra is to be desired, and it is unfortunate that an attempt to determine g at Calcutta, which was made early in 1904, ended in failure. This failure was not without interest, and I may briefly refer to it.

The whole of Calcutta may almost be said to be floating; there appears to be a crust of very moderate thickness resting on a spongy mass of unknown depth. This crust is not very rigid, and the result is that the traffic of the city sets up vibrations in it.

It had often been noticed that there was great difficulty in making nadir observations in mercury in Calcutta, but it had not occurred to me that these tremors would be of sufficiently long period to

affect a pendulum: when however the apparatus was set up, it soon appeared that they were well able to do so. A pendulum carefully brought to rest would in 20 seconds be swinging through an arc of 4' or so; this would rise and fall irregularly, but sometimes—I suppose when the incidence of traffic happened to be favourable—a large amplitude would be attained in a few minutes; I observed an amplitude of 17', which was attained in less than 10 minutes from the time when the pendulum was brought to rest. The time of vibration of the pendulum was of course quite untrustworthy as a measure of the force of gravity.

It will no doubt be possible to find places quite near Calcutta where these tremors will not be felt, but there were reasons against undertaking this at the time, and so, for the present, we have no determination of g south of Chatra, in that region.

One more point of interest belonging to this series of observations may be mentioned, namely, the visit of Dr. Hecker of the Prussian Geodetic Institute. Dr. Hecker had been making some voyages in Eastern waters to determine the force of gravity over the ocean by means of barometers and hypsometers, but he was also equipped with a set of half-seconds pendulums which he had swung at Potsdam before starting. Bringing his pendulums with him, he joined me at Jalpaiguri, and there we made simultaneous sets of swings. After this he returned to Potsdam, and again swung his pendulums, so that he has a good determination of g at Jalpaiguri in terms of g at Potsdam.

The Indian pendulums were swung at Kew and Greenwich before being taken out to India, so that we have the value of g at our base station, Dehra Dûn, in terms of g at Kew; Kew was connected with Potsdam in 1900 by Mr. G. R. Putnam; and the Indian operations connect Jalpaiguri with Dehra Dûn. Thus there is a complete circuit.

Dr. Hecker's value of g at Jalpaiguri, based on Potsdam direct, is

$$978.924 \text{ cm.}$$

The value of the same quantity obtained through Kew and Dehra Dûn is

$$978.922 \text{ cm.}$$

The agreement is satisfactory, indicating, as it does, that the Indian pendulums have not changed their lengths since they were swung at Kew.

The next series of observations which bears on the question of mountain compensation and the existence of the hidden chain of high density is one that began at Simla, and, crossing the plains of the Punjab and part of Sind, ended at Quetta.

This line includes one of the arcs of longitude which had long been a problem. The arc in question is Amritsar-Multan. At Amritsar the Himalayas are, on a fairly clear day, easily visible to

the east, while to the west there is nothing but the perfectly flat plains of the Punjab. At Multan the Suleiman Mountains close in the western horizon, and on the eastern side there is no visible source of attraction. Yet the arc of longitude showed that the plumb-lines at these two places are attracted inwards, away from the mountains and towards the level plains; and this was one of the facts utilised by Colonel Burrard to fix the position of the line of excessive density.

The pendulum stations which throw light on this problem are shown in Table II.

TABLE II.

Station.	Latitude.		Longitude.		Height.	$g''_0 - \gamma_0$.	Thickness of disc.
	°	'	°	'	feet.	cm.	feet.
Pathankot	32	17	75	39	1088	- 0'179	- 5114
Mian Mir	31	32	74	23	708	+ 0'004	+ 114
Montgomery	30	40	73	6	557	+ 0'003	+ 86
Multan	30	11	71	25	404	- 0'045	- 1287
Dera Ghazi Khan	30	4	70	46	397	- 0'088	- 2517

We have as yet no station in the Himalayas near Pathankot, nor any in the Suleiman Mountains; but since observations at Mussooree and Simla in the Himalayas and at Quetta in the Baluchistan Highlands all show defects in gravity similar to those found at Darjeeling and Sandakphu, we may surmise with fair certainty that the conditions will be found the same in the hills east of Pathankot and west of Dera Ghazi Khan.

This assumption has been made in drawing the diagrams of the excesses and defects and the ideal section on this line.

Two noteworthy points in the ideal section are (1) the great deficiency in g at Pathankot, and consequently the great depth of the "ditch" at the foot of the Himalayas; and (2) the occurrence of a similar depression or "ditch" at Dera Ghazi Khan, which lies at the foot of the Suleiman Mountains.

In the case of the mountains of Northern India, this defect of gravity at the foot is now a fairly well-established feature, for it has been found at several other places at the foot of the Himalayas, namely, at some six stations in and near Dehra-Dûn, and at Kalka, the point where the road to Simla leaves the plains; and it occurs again at Sibi, at the foot of the Baluchistan hills, among which Quetta lies.

It will be of interest to see whether the case is the same in the mountains of Southern and Central India, which are of quite different geological formation.

Some observations have been made in the Nilgiris, the Shevaroy, and the Pulnai hills, and some are in course of being made in the Vindhya ranges, but the results are not yet available.

The rise in density as we recede from the Himalayas is well shown by a series of observations on the northern part of Everest's Great Arc of Meridian. The particulars are given in Table III.

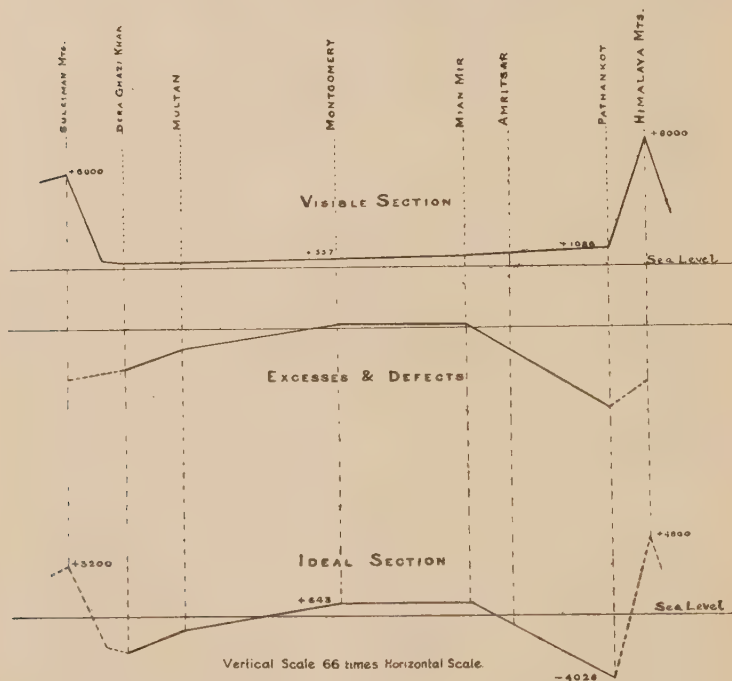


FIG. 2.

TABLE III.

Station.	Latitude.	Longitude.	Height.	$g''_0 - \gamma_0$.	Thickness of Disc.
			feet.	cm.	feet.
Mussooree .	30 27	78 3	7129	-0'115	-3289
Rajpur .	30 24	78 6	3321	-0'124	-3546
Dehra Dûn .	30 19	78 3	2239	-0'126	-3600
Asarori .	30 14	77 58	2467	-0'112	-3203
Mohan .	30 11	77 55	1660	-0'104	-2974
Nojli .	29 53	77 40	879	-0'095	-2717
Kaliana .	29 31	77 39	810	-0'058	-1659
Meerut .	29 0	77 42	734	-0'027	-772
Gesupur .	28 33	77 42	691	-0'020	-572

These stations are closer together than those of the other series that have been spoken of, and we are consequently able to form a

more precise idea of the way in which the deficiency in g diminishes as we recede from the hills.

The diagrams which follow correspond to those which illustrate the other lines of observation.

The station called Asarori is near the summit of the Siwalik range, and Mohan in the plains at its southern edge. There does not appear to be any "ditch" at the base of the Siwaliks corresponding to that which runs along the foot of the Himalayas. The

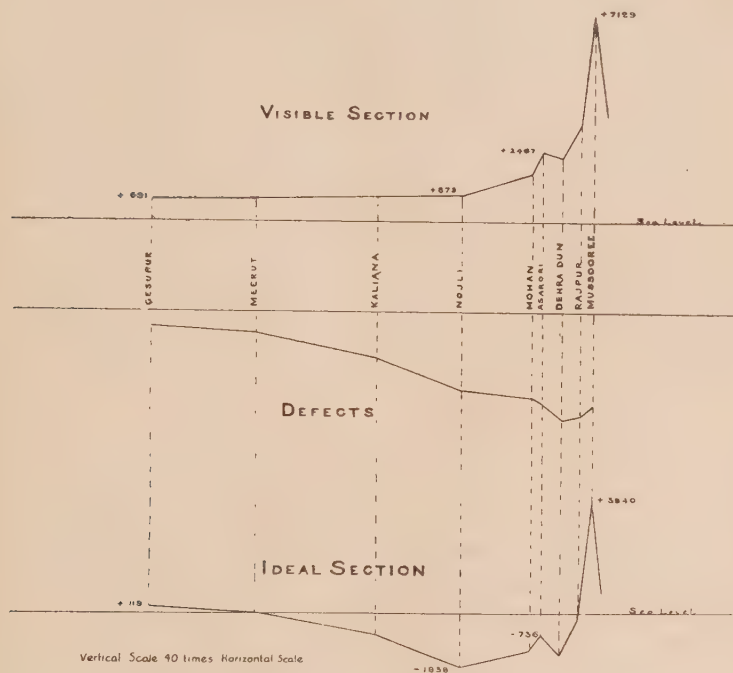


FIG. 3.

deficiency at Mohan is rather less than we should have expected if we had only had the results at Dehra Dûn and Nojli to judge by. It looks, in fact, as if in the uplifting of the Siwaliks some compression of the crust had taken place, causing a local increase of density, not indeed enough to make good the great deficiency of the sub-Himalayan belt, but sufficient to impress a distinct mark on the curve of defects.

The quantity $(g''_0 - \gamma_0)$ has its greatest negative value at Dehra Dûn, but in the ideal section Nojli is at the deepest point of the "ditch," as there is at that station only a small surplus of visible matter to set against the defect due to want of density.

From Nojli the deficiency in g''_0 diminishes very steadily ;

at Gesupur, about 100 miles south of Dehra Dûn, it amounts to 0.020; and if the diminution continues at the same rate ($g''_0 - \gamma_0$), will change sign at about 150 miles from that place.

The pendulum observations do not as yet go far enough to give any evidence as to the maximum value which ($g''_0 - \gamma_0$) will attain, nor as to the position of the crest of the invisible chain of high density.

This is the only line on which the stations are sufficiently near together to give a definite idea of the form of any part of the hidden chain, and more stations, not too far apart—say at intervals of about 20 miles—are urgently required to determine the position and height of the crest, and to reveal the section of the southern slopes.

The general conclusion to be derived from the pendulum observations, as far as they have yet gone, confirms Colonel Burrard's theory of the distribution of mass in the Himalayas and the plains below them. The mountains are compensated to some extent, but by no means wholly; and there does exist a belt of high density running parallel to the Himalayas, and some 200 or 300 miles from them. The effects, however, which Colonel Burrard attributed to the "hidden chain" must, it now appears, be ascribed only in part to that cause and in part to the deep "ditch" which lies between the hidden chain and the foot of the hills.

1909 March 3.

A proposal for the Comparison of the Stellar Magnitude Scales of the different Observatories taking part in the Astrographic Catalogue. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. One of the questions on the programme for the forthcoming meeting of the Comité Permanent for the Astrographic Catalogue (summoned for April 18-24 in Paris) is that of the best method for coordinating the measures of stellar magnitude. The different observatories have adopted different procedures, and the task of forming a homogeneous system is not by any means easy.

By the arrangement of the work, there is a zone 1° in breadth common to each pair of observatories at the separating belts; and hence there is material for a comparison of the two scales; but this implies the identification of images of the same star, which in itself involves a good deal of labour.

2. The proposal I wish to advocate is of a very simple character, and at the same time it will probably provide a sufficiently good comparison of the scales, with comparatively little labour. It is—*That the number of images recorded under each unit of the magnitude scale be counted and tabulated.*

The illustrations which follow will be sufficient to show the advantages to be gained by this simple process. They are to be regarded as illustrations merely, and not as definitive results; and they are set down rather hurriedly at this moment, in order to have some information ready for the Committee.

The Oxford Results.

3. Naturally, we have paid most attention to our own results, and these may be given first. At Oxford the diameters of the images are estimated in units of $0''.3$, allowance being made for the greyness of the images of the fainter stars. But it was difficult to lay down exact rules in this respect, and the various measurers have differed a good deal in their practice. It is thus natural to treat their measures separately; and the way in which their idiosyncrasies are made apparent is a very good illustration of the value of the method.

TABLE I.
Measures of B.G. in Zone $+30^\circ$.

<i>d.</i>	<i>n₀.</i>	<i>log n₀.</i>	<i>log n_c.</i>	<i>O₁-C₁.</i>	<i>Dec. Eqn.</i>	<i>O₂-C₂.</i>
15	818	2.91	3.07	- .16	+ .24	+ .08
16	579	2.76	3.01	- .25	+ .30	+ .05
17	471	2.67	2.94	- .27	+ .18	- .09
18	458	2.66	2.87	- .21	+ .22	+ .01
19	499	2.70	2.80	- .10	+ .17	+ .07
20	1035	3.01	2.72	+ .29	- .36	- .07
21	603	2.78	2.65	+ .13	- .18	- .05
22	762	2.88	2.59	+ .29	- .31	- .02
23	407	2.61	2.52	+ .09	- .05	+ .04
24	186	2.27	2.46	- .19	+ .14	- .05
25	118	2.07	2.40	- .33	+ .24	- .09
26	100	2.00	2.34	- .34	+ .30	- .04
27	155	2.19	2.28	- .09	+ .18	+ .09
28	101	2.00	2.22	- .22	+ .22	.00
29	86	1.93	2.17	- .24	+ .17	- .07
30	362	2.56	2.12	+ .44	- .36	+ .08
31	204	2.31	2.07	+ .24	- .18	+ .06
32	226	2.35	2.01	+ .34	- .31	+ .03
33	92	1.96	1.95	+ .01	- .05	- .04
34	65	1.81	1.90	- .09	+ .14	+ .05

4. In Table I. are given the number of times (n_0) on which B.G. recorded the value of d given in the first column, between $d=15$ and $d=34$. Outside these limits the numbers become small or irregular, and we need not notice them at present. In the third column we take the logarithm of n_0 , which is the suitable function, since we are clearly dealing with percentages, *i.e.* with $\Delta n_0/n_0$, the integral of which is $\log n_0$. In the fourth column values of $\log n_0$ are given from a smooth curve, and it will be obvious that the residuals in the fifth column originate with the observer, and not with the stars. The most conspicuous tendency is to record terminal digits 0-3 rather than 4-9.

In fact B.G. has a well-marked "decimal equation," which we may exhibit as follows:—

TABLE II.

B.G.'s Decimal Equation for Zone +30°.

Terminal Unit.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
O—C 15-24	+ '29	+ '13	+ '29	+ '09	- '19	- '16	- '25	- '27	- '21	- '10
O—C 25-34	+ '44	+ '24	+ '34	+ '01	- '09	- '33	- '34	- '09	- '22	- '24
Mean	+ '36	+ '18	+ '31	+ '05	- '14	- '24	- '30	- '18	- '22	- '17

5. The two sets differ a little, and indeed there is no reason why an observer should treat 15 and 16 exactly as he treats 25 and 26. But it is obvious that the decimal equation is the main feature of his idiosyncrasy; and on correcting for the mean, as in column 6 of Table I., we get the much improved residuals of column 7. These suggest a correction to the curve, but the complete discussion would take us too far for the present purpose, which is merely to make clear that these counts can be profitably studied.

6. As a second example we will consider the whole of B.G.'s measures (not merely those between $d=15$ and $d=34$) in different hour angles; but to give every separate value of d is unnecessary at present. We may group values of d as in Table III., the groups corresponding to magnitudes increasing uniformly, if we adopt the formula (used at Greenwich and Oxford),

$$\text{mag.} = a - b \sqrt{d}.$$

Further, instead of using n , the number under each heading, it is more convenient to use N , the number brighter than and including that heading; or rather $\log N$, as Kapteyn has already done. The details will be clear from Table III.

7. The first column gives the values of the diameter d , the second the mean value of $\sqrt{d}$, which decreases with tolerable uniformity by about 0.4 each time, corresponding to half-a-magnitude.

TABLE III.
B.G.'s Measures of Diameter in Zone +30°.

<i>d.</i>	$\sqrt{d.}$	R.A. $0^h-4\frac{1}{2}^h$			R.A. $4^h57'-6^h36'$			$\log N_2 - \log N_1$	Diff. from Mean.	Approx. Mag.
		n_1	N_1	$\log N_1$	n_2	N_2	$\log N_2$			
50 and over	> 7.1	13	13	1.11	23	23	1.36	+0.25	+ .02	> 7.0
45 to 49	6.8	2	15	1.18	11	34	1.53	+0.35	+ .12	7.3
40 „ 44	6.5	22	37	1.57	36	70	1.84	+ .27	+ .04	7.7
35 „ 39	6.1	11	48	1.68	16	86	1.93	+ .25	+ .02	8.2
30 „ 34	5.6	65	113	2.05	101	187	2.27	+ .22	- .01	8.7
26 „ 29	5.2	36	149	2.17	53	240	2.38	+ .21	- .02	9.2
22 „ 25	4.8	100	249	2.40	125	365	2.56	+ .16	- .07	9.7
18 „ 21	4.4	131	380	2.58	252	617	2.79	+ .21	- .02	10.2
15 „ 17	4.0	93	473	2.67	217	834	2.92	+ .25	+ .02	10.7
12 „ 14	3.6	298	771	2.89	575	1409	3.15	+ .26	+ .03	11.2
9 „ 11	3.2	565	1336	3.13	762	2171	3.34	+ .21	- .02	11.7
7 and 8	2.7	115	1451	3.16	33	2204	3.34	+ .18	- .05	12.0
5 „ 6	2.3	1	1452	3.16	0	2204	3.34	+ .18	- .05	12.2

The third column gives the number of records in each group for plates in R.A. $0^h-4\frac{1}{2}^h$, away from the Milky Way.

The fourth column is formed by continual addition of the items of the third, and in the fifth the logarithm is taken.

The sixth, seventh, and eighth columns show similar figures for plates in the Milky Way.

8. In the ninth column the differences of $\log N$ are taken, and in the tenth the residuals from a mean of +0.23. This mean difference signifies nothing beyond the fact that more measures were made in the second set than in the first, and it may be neglected. The tenth column shows us, on the hypothesis that the irregularities of B.G.'s scale have been eliminated by the subtraction, how the stars increase in the Milky Way compared with the increase in a non-Galactic region. Now these numbers are no doubt rough: they depend on a few plates only, measured by a measurer with strongly marked bias; but they seem to indicate that $\log N_2 - \log N_1$ does not *increase* for the fainter stars, as Kapteyn has found. The approximate magnitude corresponding to the diameter is given in the eleventh column, and we may extract from Kapteyn's recent important paper (No. 18 of the *Groningen Publications*) the following values for $\log N$ in the Galaxy and out of it (Table IV.). We see that the difference for Galaxy-non-Galaxy increases by 0.138 in 4 magnitudes, whereas we get a decrease if anything.

TABLE IV. (*Kapteyn*).

Mag.	$\log N_{0.20}$	$\log N_{30.40}$	Diff.
8.0	0.199	9.998	+ .201
9.0	0.696	0.468	+ .228
10.0	1.187	0.924	+ .263
11.0	1.668	1.368	+ .300
12.0	2.138	1.799	+ .339

9. So marked a difference from Kapteyn seemed worth checking by the independent measures of another observer in another zone ; and accordingly the measures of a few plates by T.M. in zone $+29^{\circ}$ were taken as below.

TABLE V.

Log N from Measures by T.M. in Zone $+29^{\circ}$.

Approx. Mag.	Galaxy.	Non.-Gal.	Diff.	Add '16.	B.G. from Table III.
<7'0	1'23	1'53	- '30	- '14	+ '02
7'3	1'49	1'61	- '12	+ '04	+ '12
7'7	1'67	1'80	- '13	+ '03	+ '04
8'2	1'86	1'96	- '10	+ '06	+ '02
8'7	2'04	2'19	- '15	+ '01	- '01
9'2	2'15	2'34	- '19	- '03	- '02
9'7	2'32	2'50	- '18	- '02	- '07
10'2	2'57	2'71	- '14	+ '02	- '02
10'7	2'69	2'84	- '15	+ '01	+ '02
11'2	2'93	3'08	- '15	+ '01	+ '03
11'7	3'09	3'26	- '17	- '01	- '02
12'2	3'23	3'40	- '17	- '01	- '05
12'7	3'40	3'53	- '13	+ '03	

10. Table V. needs little explanation : log N is formed in the second column for plates in the Galaxy, and in the third for plates away from it. The differences in column 4 should increase, but they remain very nearly constant ; such slight residuals as are shown in column 5 having several features in common with the corresponding numbers of B.G., and confirming the decrease, rather than increase, found before.

11. Such a cursory examination is of course insufficient for any purpose beyond that of demonstrating the importance of discussing these star counts. We see that, defective as B.G.'s magnitude scale is from many points of view, it still serves to compare different parts of the sky when it is converted into the scale of log N.

12. But it may be said that the Oxford measures are exceptional, and that other observatories have used magnitude scales which do not show idiosyncrasies of the same kind. Accordingly, the results of a few other observatories have been examined in a purely provisional manner, and the outcome is given below.

The Greenwich Results.

13. The Greenwich methods are very similar to the Oxford methods, with one exception in detail. Two measures of diameter are made, in reversed positions of the plate : at Oxford we print the mean of the two, but at Greenwich the sum is printed. Now this leads to

a curious result. In Table VI. are given counts of the Greenwich stars for the zone $+64^\circ$, for different values of d . The divisions of the groups are of no particular significance, the meaning being simply that a page of computations was filled up and another begun: but the groups follow in order, and are kept separate in order to show the persistence of the curious feature to which attention is now called, viz. that there are very few *odd* values of d , almost all the tabulated sums being even.

TABLE VI.

Greenwich Values of Diameter: Zone $+64^\circ$.

d .	I.	II.	III.	IV.	Total.	d .	I.	II.	III.	IV.	Total.
4	3	3	20	40	66	16	48	56	66	41	211
5	3	2	4	59	68	17	13	10	6	17	46
6	29	19	63	130	241	18	45	52	58	43	198
7	16	27	11	38	92	19	8	7	6	18	39
8	51	46	122	117	336	20	41	52	47	35	175
9	17	13	22	35	87	21	10	5	8	17	40
10	85	100	130	81	396	22	27	34	44	21	126
11	15	14	9	27	65	23	8	6	6	10	30
12	66	65	118	84	333	24	28	30	36	36	130
13	16	9	7	27	59	25	8	6	5	7	26
14	62	70	90	61	283	26	33	23	31	23	110
15	11	10	12	26	59	etc.					

14. Such a peculiarity may arise in various ways, of which two at least are worthy of attention.

(a) The measurers may have a tendency to put down even numbers only, in which case the sum will of course be even.

(b) The two measures may not be independent. If there is any tendency to repeat the first in the second, the sum will of course tend to be even.

15. In order to see whether (a) is possibly a general habit, sums of the two values of d were formed for a few Oxford plates measured by different people, and the number of odd sums compared with the number of even sums. The results are given in Table VII.

TABLE VII.

Comparison of Odd and Even Sums of d for Oxford Measures.

Measurer.	Odd Sums.	Even Sums.
H.T.	70	70
F.B.	185	197
B.G.	69	176
E.G.	261	257

The only observer showing a tendency to even sums is B.G. ; and his separate entries were then examined. It was found that he made 370 even entries as against 127 odd, so that he comes under head (a) above.

16. The measurers of the Greenwich plates are not indicated in the volume, so that it is not possible to study their relative habits. But this could be done at the observatory, where records are doubtless kept; and probably results of interest would be obtained in this way.

The Potsdam Results.

17. The Potsdam results have been recorded in magnitudes. Counts have been made on the plates in vol i., which are in miscellaneous order; and only the general nature of the scale is manifested. The subdivision is practically into quarter magnitudes, as shown in the first column of Table VIII. In the next six columns are given separate results for six consecutive groups of plates, in order to show the persistence of the special features, which are seen more clearly in the 8th. column of totals, where it will be noticed that the numbers are alternately large and small. Nevertheless, log N shown in the 10th. column agrees fairly well with Kapteyn's log N_0^{90} , when a constant 2.30 is added to secure approximate adjustment.

TABLE VIII.

Potsdam Results.

Mag.	I.	II.	III.	IV.	V.	VI.	Total n.	N.	log N.	Kapteyn log N_0^{90} +2.30	P-K.
<8.0	21	36	19	11	8	17	112	112	2.05		
8.0	5	39	7	13	9	13	86	198	2.30	2.35	- .05
8.2	6	25	10	4	3	9	57	255	2.41	2.47	- .06
8.5	16	51	26	23	7	12	135	390	2.59	2.59	.00
8.8	14	28	14	11	8	6	81	471	2.67	2.71	- .04
9.0	69	128	39	43	17	39	335	806	2.91	2.83	+ .08
9.2	35	89	27	34	13	23	221	1027	3.01	2.95	+ .06
9.5	110	149	29	40	39	28	395	1422	3.15	3.06	+ .09
9.8	59	107	46	44	13	35	304	1726	3.24	3.18	+ .06
10.0	164	170	74	79	47	72	606	2332	3.37	3.30	+ .07
10.2	33	115	55	52	25	40	320	2652	3.42	3.42	.00
10.5	119	173	121	127	76	90	706	3358	3.53	3.53	.07
10.8	32	92	61	76	23	51	335	3693	3.57	3.64	- .07
11.0	96	174	216	190	168	172	1016	4709	3.67	3.75	- .08

18. There is apparently a rather sudden discontinuity in the Potsdam scale near magnitude 9.0, and a tolerably sharp drop after magnitude 10.0.

19. It seems worth inquiring whether the Potsdam results show

differences between the Galaxy and the non-Galaxy similar to those shown at Oxford.

Group II. is roughly a Galactic region, and groups IV. and V. are non-Galactic.

TABLE IX.
Potsdam Results.

Mag.	Galactic.			Non-Galactic.			$\log N_2 - \log N_1$	Sub. 0.27.
	n_2 .	N_2 .	$\log N_2$.	n_1 .	N_1 .	$\log N_1$.		
< 8.0	36	36	1.56	19	19	1.28	+ 0.28	+ .01
8.0	39	75	1.88	22	41	1.61	+ .27	.00
8.2	25	100	2.00	7	48	1.68	+ .32	+ .05
8.5	51	151	2.18	30	78	1.89	+ .29	+ .02
8.8	28	179	2.25	19	97	1.99	+ .26	- .01
9.0	128	307	2.49	60	157	2.20	+ .29	+ .02
9.2	89	396	2.60	47	204	2.31	+ .29	+ .02
9.5	149	545	2.74	79	283	2.45	+ .29	+ .02
9.8	107	652	2.81	57	340	2.53	+ .28	+ .01
10.0	170	822	2.91	126	466	2.67	+ .24	- .03
10.2	115	937	2.97	77	543	2.73	+ .24	- .03
10.5	173	1110	3.05	203	746	2.87	+ .18	- .09
10.8	92	1202	3.08	99	845	2.93	+ .15	- .12
11.0	174	1376	3.14	358	1203	3.08	+ .06	- .21

Forming $\log N$ for these portions, and taking the differences in the sense Galaxy-non-Galaxy, the differences certainly do not seem to increase. We thus seem to find an essential difference from Kapteyn's results. Is it due to our using photographic magnitudes instead of visual? The point is well worth careful investigation, and it seems clear that a good deal can be learnt from these simple counts, although the magnitude scales may be defective in different ways. It seems quite possible that we are here in presence of a physical fact connected with the scattering of light in space, as outlined last November (*Monthly Notices*, vol. lxix. p. 61). It was pointed out that a crucial test of such scattering would be to photograph the stars in visual light, and compare the increase in time of exposure with that required for violet light. M. Tikhoff has since applied this test with success (see *Comptes Rendus* for February 1), and considers this differential effect established; so that we may expect fundamental differences between visual and photographic magnitudes, and the point above noted may be one manifestation of them.

The Toulouse Results.

20. Volume ii. was selected at random, containing the results in $+9^\circ$; and counts were made of the plates in the order of R.A.

The first set of plates in Table X. represent earlier R.A.'s, and are therefore further from the Galaxy; the second set are nearer the Galaxy, though they stop short of it. The differences of $\log N$ here show a distinct increase from magnitude 8.0 to 10.0, but then decrease again. It seems probable that a much more extensive investigation is needed to get at the facts clearly.

21. As regards the Toulouse scale, it will be seen that the values of N for 8.0, 9.0, 10.0, and 11.0 show excesses over their neighbours; and there is doubtless some reason for this idiosyncrasy. But the reason can be most profitably studied at the observatory itself, when once these counts have been made and tabulated.

TABLE X.

Toulouse Results.

Mag.	Group I. Non-Galactic.			Group II. Near Galactic.			$\log N_2 - \log N_1$	Add 0.20.
	n_1 .	N_1 .	$\log N_1$.	n_2 .	N_2 .	$\log N_2$.		
< 8.0	73	73	1.86	39	39	1.59	- 0.27	- .07
8.0	28	101	2.00	20	59	1.77	- .23	- .03
8.2	6	107	2.03	4	63	1.80	- .23	- .03
8.5	20	127	2.10	14	77	1.89	- .21	- .01
8.8	27	154	2.19	14	91	1.96	- .23	- .03
9.0	62	216	2.33	44	135	2.13	- .20	.00
9.2	37	253	2.40	23	158	2.20	- .20	.00
9.5	51	304	2.48	56	214	2.33	- .15	+ .05
9.8	72	376	2.58	42	256	2.41	- .17	+ .03
10.0	117	493	2.69	121	377	2.58	- .11	+ .09
10.2	77	570	2.76	49	426	2.63	- .13	+ .07
10.5	155	725	2.86	80	506	2.70	- .16	+ .04
10.8	162	887	2.95	67	573	2.76	- .19	+ .01
11.0	434	1321	3.12	271	844	2.93	- .19	+ .01
11.2	380	1701	3.23	186	1030	3.01	- .22	- .02
11.5	474	2175	3.34	261	1291	3.11	- .23	- .03
11.8	482	2657	3.42	287	1578	3.20	- .22	- .02
12.0	376	3033	3.48	260	1838	3.26	- .22	- .02
12.3	533	3566	3.55	523	2361	3.37	- .18	+ .02

The Helsingfors Results.

22. The results have been reduced to a magnitude scale which shows great smoothness to magnitude 10.6 or 10.7. The numbers then fall off, indicating a systematic error for the faintest stars. The plates counted were those at 6^h 0^m for zones + 40°, + 42°, + 44°, and

+ 46°, which are near the Galaxy; and 10 plates from 8° 35^m + 45° to 8^h 50^m + 42°, which are at some distance away. The differences $\log N_2 - \log N_1$ show no increase between 8.5 and 11.0; and though there is a curvature shown, it is in the opposite direction to that of the Toulouse results.

TABLE XI.

Helsingfors.

Mag.	Galactic.			Non-Galactic.			$\log N_2 - \log N_1$.
	n_2 .	N_2 .	$\log N_2$.	n_1 .	N_1 .	$\log N_1$.	
to 8.5	85	85	1.93	75	75	1.88	+ 0.05
8.6	17			20			
8.7	21			11			
8.8	29			23			
8.9	32			26			
9.0	27	211	2.32	30	185	2.27	+ 0.05
9.1	25			47			
9.2	36			53			
9.3	21			49			
9.4	37			37			
9.5	30	360	2.56	45	416	2.62	- 0.06
9.6	36			33			
9.7	47			34			
9.8	41			46			
9.9	44			46			
10.0	41	569	2.76	35	610	2.79	- 0.03
10.1	41			43			
10.2	47			37			
10.3	50			55			
10.4	69			56			
10.5	77	853	2.93	57	858	2.93	0.00
10.6	94			81			
10.7	91			43			
10.8	69			73			
10.9	76			67			
11.0	55	1238	3.09	43	1165	3.07	+ 0.02
11.1	74			34			
over 11.1	34			39			

The Paris Results.

The scale used is one of magnitudes, but the following figures will show that the scale is not uniform. They refer to the first four plates in zone $+23^{\circ}$.

TABLE XII.

Mag.	n.	Mag.	n.
< 9.0	108		
9.1	14	10.1	54
9.2	22	10.2	56
9.3	31	10.3	26
9.4	18	10.4	29
9.5	12	10.5	34
9.6	18	10.6	188
9.7	48	10.7	20
9.8	28	10.8	29
9.9	26	10.9	33
10.0	37	11.0	253

Summary.

1. It is suggested that the observatories taking part in the Astrographic Catalogue shall make systematic counts of the numbers of images shown under each division of their magnitude scales.

2. These counts will generally indicate systematic errors in the scales which can be best investigated at the observatory, since some of them may depend on conditions not recorded in the published volumes—change of measurer, for instance.

3. But with such counts before us, even without systematic correction, it is possible to get important information as to the relation of one part of the sky to another.

4. In particular, Kapteyn finds the Galaxy relatively richer in faint stars than the non-Galactic parts of the sky. Counts on the Oxford, Potsdam, Toulouse, and Helsingfors plates fail to support this conclusion. This seems to point to a fundamental difference between visual and photographic magnitudes, possibly due to scattering of light in space.

Some Notes on Aberration. By H. H. Turner, D.Sc. F.R.S.,
Savilian Professor.

1. It may be stated at the outset that the net result of the following discussion is to come back to the starting-point. It is the account of a train of thought, in the course of which a new departure was suggested more than once, only to prove misleading; and the only reason for printing the account is that it may possibly save others from some of the pitfalls which were noticed (generally by falling into them). It may seem presumptuous to think that others will not be more wary; but on at least one occasion I have found the opinion of others, familiar with the subject, to agree with mine, although this has ultimately proved erroneous. I do not wish, by saying this, to shirk responsibility for the mistakes made, but only to strengthen the case for printing this record of certain seductive errors and their exposure.

2. The keynote of the discussion is the following question, to which my attention was originally drawn by reading a most interesting chapter in Mr. N. R. Campbell's book, *Modern Electrical Theory*, Camb. Un. Press, 1907.

Does the correction for aberration in the position of an observed heavenly body P depend on the velocity of the observer *relative to* P , or upon his absolute velocity in space?

Our instinct is to reject the second alternative, because our knowledge of all motion can only be relative. Even the introduction of the notion of the ether, which enables us to talk of the Earth's motion "relatively to the ether," instead of the "absolute motion in space," is not altogether satisfactory. But if aberration depends on the relative motion of observer and star, into which the motion of the star enters just as much as that of the Earth, how is it that we correct for the motion of the Earth in its orbit, and neglect the motion of the star which may also be moving in an orbit? At first sight it certainly seems as though we are concerned specially with the absolute motion of the Earth, or something very like it.

3. But the difficulty is to devise a test which shall distinguish between the two hypotheses. This looks at first sight an easy matter, but it is not so, as will presently appear. The following test suggested itself to me, and was judged sound by others:—

Let $A\ C\ B$, fig. 1, be the equatorial section of the Sun; A and B the positions of a spot when it is just appearing and just disappearing; C its position on the centre of the disc. If aberration depends on relative velocity (*i.e.* on the velocity of the source of light just as much as on that of the recipient, only in the reverse direction of course), then the velocity of the spot across the line of sight at C should displace it aberrationally, while at A and B there would be no effect of that kind. Hence, if the heliographic longitude of the spot observed at C be compared with the mean of those at A and B ,

it will be found to be displaced. To assess the amount, we may compare with the $20''$ due to the Earth's annual motion. The velocity of C is at a radius $1/200$ that of the Earth's orbit, but C

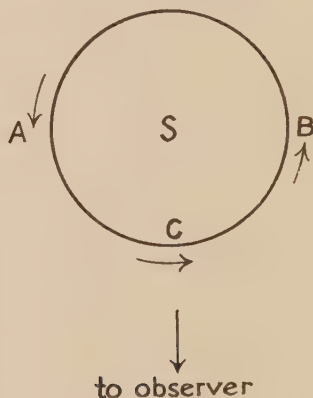


FIG. 1.

revolves in 25 days instead of 365; hence the amount should be $15 \times 20''/200 = 1''.5$ approximately.

4. This is the effect to the observer, and is independent of his distance, just as the aberration correction for the stars is independent of their distance. When the observer is on the Earth, $1''.5$ corresponds to $200 \times 1''.5 = 5' = 0.08$ of solar longitude. Now this is a small quantity, but my experience of the accuracy of the Greenwich sun-spot measures led me to think that it could be detected in the mean of a large number of observations; and as support was forthcoming for the view that the result would answer the question of § 2, the computations were undertaken in the Computing Bureau of the I.U.S.R. The longitudes of a large number of spots, as observed in the centre of the disc, were compared with the longitudes found for the same spots near the limb. It was not, of course, possible to use measures of the spots actually in the positions A and B of fig. 1, but positions near these were available. and in practice they were distant 65° from the central meridian. [To get good values of the longitudes at these points the means of values at 75° , 65° , and 55° were taken, by a sufficiently approximate process of interpolation.] The mean of the observed longitudes at $+65^\circ$ and -65° from the central meridian was then compared with that on the central meridian [also obtained from three consecutive days.] Only those spots were used which travelled completely across the disc; but all such spots as were available in the thirty-two years 1874-1905 were used.

5. The results are as follows, the quantity tabulated being the excess of the observed longitude in the centre of the disc over that near the edges.

Excess of observed Longitude on central Meridian over that at $\pm 65^\circ$ from centre for Greenwich spot-groups 1874-1905.

Latitudes of Spots.	No. of Spots.	Observed Excess.
over -25	20	$-0^\circ 43$
-20 to -25	41	$+0^\circ 15$
-15 to -20	97	$-0^\circ 06$
-10 to -15	103	$-0^\circ 14$
-0 to -10	98	$-0^\circ 04$
$+0$ to $+10$	74	$-0^\circ 19$
$+10$ to $+15$	125	$+0^\circ 01$
$+15$ to $+20$	57	$-0^\circ 25$
$+20$ to $+25$	31	$-0^\circ 24$
over $+25$	11	$+0^\circ 16$
Total	657	$-0^\circ 10$ simple mean.
		$-0^\circ 09$ weighted „

Weighted mean for spots between $+15^\circ$ and -15° of latitude $-0^\circ 08$.

6. Since the velocity of the source should throw the object *back* (that of the Earth throws it apparently *forward*), the sign of the result accords with an aberration effect. As regards the amount, we must correct the quantity found above for the fact that outer observations were not on, but only near the limb, so that we have got only a portion of the total effect.

The reducing factor depends both on the latitude and on the distance (65°) in longitude from the central meridian; but the effect of the latitude is small, and it will be sufficient for our present purpose to take the factor as $(1 - \cos 65^\circ) = .58$; so that for the total aberration effect we should find (from the weighted mean)

$$-0^\circ 09 / .58 = -0^\circ 15.$$

This is nearly double the amount computed above ($0^\circ 083$), but is of the same order of magnitude, and seemed to afford some confirmation of the view that the velocity of the source of light enters into the aberration effect.

7. But while this work was being done, it occurred to me that there were serious difficulties attending the view that the velocity of the source of light could produce an aberration effect when we apply the principle to the stars. Aberration is an angular displacement independent of the distance of the body observed, whereas the actual movements of the stars are diminished by distance. Consequently, in the case of double stars, we should find the aberration effect increasing in importance relatively to the apparent size of the orbit as the orbit was more and more remote.

8. Suppose, for instance, there was a binary composed of two equal stars revolving in a year at the distance separating the Earth

and Sun, in a plane through the line of sight. Then, when they were actually passing through the line of sight, they would be displaced by $20''$ in opposite directions, or separated to the extent of $40''$ to our vision (as seen some time subsequently). There is no visual binary with a period so short as a year, but there are three with periods under 20 years, and their maximum separations are all under $1''$. Even if we ascribe the whole of this separation to the aberrational effect, we must attribute very small velocities, and therefore very small masses, to the components, and this does not accord with other known facts. Still greater difficulties occur in the case of the spectroscopic binaries which have known linear velocities in the line of sight, implying transverse velocities of similar magnitudes, and yet the components are not seen separated. It would seem that the velocity of the source of light cannot therefore enter into the aberration.

9. A more general difficulty was pointed out by a correspondent after consideration of the above instances. Accepting the result that the centre of the Sun's disc is displaced by $1''.5$ to an observer at any distance, it follows that if the Sun were at a stellar distance its light would be spread over an arc $1''.5$ in length, instead of being a stellar point. It is doubtful whether we should see it at all in that case, and, as my correspondent remarked, the only stars we should see would be those which do not rotate.

10. We seemed, therefore, to be in the position that a definite test applied to the Sun supported the view that the motion of the source *does* enter into the aberration; while the application of this principle to the stars requires an almost unthinkable interpretation of observed facts.

11. I now proceed to show that both of these arguments are

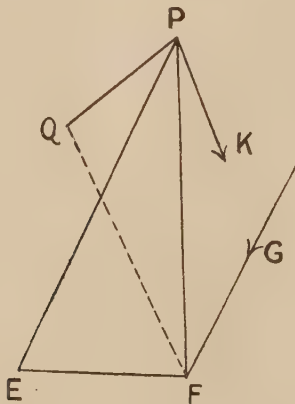


FIG. 2.

fallacious. Let us first take the simple case of the Earth and a planet, where the motions are known relatively to the Sun, and the

time of light transmission is so small that the paths of the bodies may be taken as straight for the interval. Let P (fig. 2) be the position of the planet when the light leaves it at time T , and E the position of the Earth. The light reaches the Earth at a time t , when the Earth is at F and the planet has moved to Q, so that the light travels along the straight line PF. But on reaching the Earth at F it *appears* to come in the direction GF, owing to the aberration effect discovered by Bradley, which depends on compounding the velocities of the Earth and light; and since EF and PF are proportional to these velocities, GF is parallel to PE. Hence at the time T the planet actually was in the direction EP from the Earth in which it *appears* to be at the time t , and we get the well-known principle of "antedating" the observation by the interval $t - T$.

12. Now in this procedure we take no account of the velocity PQ of the planet, and it is therefore at first sight difficult to believe that anything can depend on the relative velocity of the Earth and planet. But let us now consider the state of things at the other end P, and see how the velocity of the planet enters into the question. The light which reaches the Earth leaves the planet in the direction PF; but just as this direction does not coincide with that in which it *appears* to reach the Earth, so, owing to the planet's velocity, it does not coincide with that in which it *appears* to leave the planet. If we wish to receive the light at F, we must point a telescope in the direction FG; and so, if we wish to transmit the light at P, we must point the discharging gun in the direction PK. The gun must fire with the velocity of light, and a better metaphor can be obtained by keeping to the telescope. To send light to the Earth, we may conceive that there is on the planet a telescope with a source of light in its focus which sends light through the object-glass to the Earth in strictly parallel rays. This telescope must be pointed, not in the direction PF in which the light actually travels, but in the direction PK, which is easily seen to be parallel to QF, by the composition of the velocities of the planet and light, just as FG is parallel to EP by the composition of the velocities of the Earth and light.

13. We can now see what happens when we use the *relative* velocity of Earth and planet in correcting for aberration, instead of the velocity of the Earth alone.

If we correct the observed direction GF for the velocity of the Earth alone, we get the direction PF.

If we further correct this direction PF for the velocity of the planet, we get the direction PK, which may be called the observed direction of transmission.

But it is easily seen that the velocity of the planet is applied in the opposite direction, so that the total correction is of the form earth minus planet, and thus depends directly on the relative velocity of Earth and planet.

Further, the direction PK is parallel to QF, which is the direction in which the planet actually lies from the Earth at the moment of observation t .

Hence by applying to the observation a correction for aberration depending on the relative velocity of the Earth and planet, we get the actual position of the planet at the moment of observation. Thus the view that the aberration correction depends on relative velocity is completely justified.

14. We have then two alternatives in procedure :—

(α) We may accept the observation as it stands, as representing the position of the planet from the Earth at time T when the light started.

(β) We may correct the observation for the relative velocity of the Earth and planet, and we then obtain the position of the planet at time t when the observation is actually made.

15. These alternatives (with a third to be presently noticed) are at least as old as Gauss, and have been repeated from the *Theoria Motus* into various text-books, for the references to which I am indebted to Mr. H. C. Plummer. But in no case that we have been able to find is the point brought out that in (β) we use essentially the *relative* motion of Earth and planet, which is a conception of importance physically. Take, for instance, the following version of (β) from Chauvenet, vol. i. p. 642.

“II. The true place and distance being known for the time t , we compute the reduction t — T . Thus by means of the diurnal motion of the planet (in longitude and latitude, or in right ascension and declination) we can reduce the true place from the time t to the time T ; and the true place thus found is the apparent place at the time t .”

This is a clear statement of the computational method by which the principle of (β) is put into practice; but the method of statement obscures the principle itself, and would scarcely convey to the physicist who is unfamiliar with our computational procedure that the net result was to apply an aberration depending on the relative velocity of Earth on planet. Consequently this essential point has been somewhat lost sight of, and I therefore venture to recall attention to it.

16. The third alternative mentioned by Gauss and his successors makes use of the line PF in fig. 2, the position of the planet at time T , as seen from the Earth at time t . Now it seems worth while to remark on the essential difference between PF and the lines PE and QF which are used above in (α) and (β) respectively. The points P and E represent simultaneous positions of planet and Earth; and the direction PE is thus unaffected by the motion of the whole system, whereas PF depends on that motion. To bring out this point, let us make the simple supposition that the whole solar system happens to be travelling in space with a velocity equal and opposite to that of the planet, so that P is really at rest in space, or in the ether. Then we must substitute for fig. 2 a new construction, as in fig. 3. The Earth now moves along EK , compounded of EF and FK , equal to QP . Hence the light travels, not along PF , but along PK . Its path depends in fact upon the assumed velocity of the solar system as a whole. But EP , the

will presently return to it, and show that it is too small to be sensible. Neglecting it for the present, let G H A B C D in fig. 4 represent the Sun's disc as seen from the Earth E. Then an observation made when the spot is actually at B gives us its true direction, as seen from E, 8 minutes earlier, when it actually was

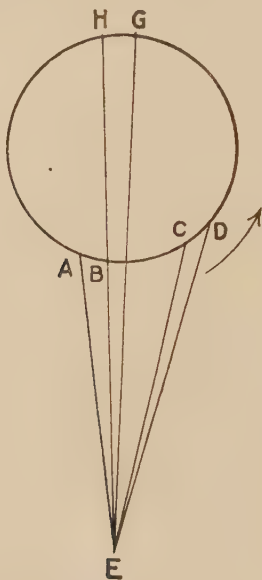


FIG. 4.

at A. Similarly, when the spot is actually at D we observe it at C; when actually at H we observe it at G; and the arcs A B, C D, G H are all equal if we neglect the small correction above mentioned. Taking the light-time as 8 minutes, and the time of a rotation of the Sun as 25 days, each of these little arcs is in degrees

$$\frac{8 \times 360T}{25 \times 24 \times 60} = 0^{\circ}.08,$$

which is the quantity calculated in § 4. But we see, further, that this correction *when expressed in solar longitude* is applicable equally in all positions of the spot, and not specially to those in the centre of the disc. The velocity across the line of sight, on which the aberration effect depends, no doubt diminishes as we pass from the centre of the disc to the limbs, the reducing factor being the cosine of the longitude from the central meridian; but when we convert the measured position into solar longitude, we multiply by the secant of this same angle, which cancels out the cosine and leaves us a constant effect. The result found in § 6

cannot, therefore, be due to a neglected aberration correction, and the explanation of it must be sought elsewhere.

18. As regards the small correction for the distance of the limbs beyond the centre of the disc, the time taken by light to traverse the Sun's radius is $2\frac{1}{2}$ seconds only, and the effect on the longitude is $0^{\circ}.004$. This correction is theoretically applicable to solar photographs, but it is clearly quite insensible in the present state of observation.

19. We now come to the case of a star when the time T taken by the light to travel from star to Earth may be very large—thousands of years perhaps—and the Earth's path cannot be regarded as a straight line, as drawn in fig. 2. We can, however, re-interpret fig. 2 as follows.

P is still the position of the star when the light leaves it.

F is still the position of the Earth when the light is received.

GF is still the direction in which the light seems to come.

EF is still the direction of the Earth's motion at the moment of observation.

But E is no longer the point where the Earth actually was when the light started. It is the point where it *would have been* if it had been travelling since the time T with the velocity it has at the moment of observation.

And consequently EP is the direction in which the star P lay, from this fictitious Earth, at the time when the light started.

20. Now it is inconvenient to refer the positions of P to such a fictitious Earth; for this fiction describes a large orbit in space. Let $F_1 F_2 F_3$ in fig. 5 represent the actual orbit of the Earth about

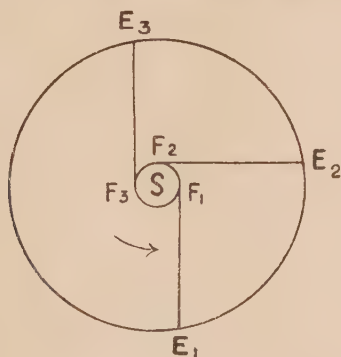


FIG. 5.

the Sun S . Then an observation made at F_1 refers the star to a fictitious Earth E_1 , $E_1 F_1$ being the distance the Earth would travel in many years: an observation at F_2 refers to E_2 , and so on. The variation in the Earth's velocity causes the fictitious Earth to describe a huge orbit $E_1 E_2 E_3$. This fictitious Earth is consequently an inconvenient point of reference, and we accordingly do not use

it. We correct the observations for the variable part of the Earth's velocity in the well-known manner, and the observations are then referred to the Sun.

21. But the Sun is also moving, and we have still to consider the effect of his movement in space. The Sun now takes the place of the Earth in fig. 2: the observation being made at F_1 (and corrected, as above, for that part of the aberration which depends on the Earth's annual motion), we get the position in which the star P lay from the Sun at a previous time T, if the Sun's velocity has been uniform during this time. If it has not been uniform, the observation is referred to a fictitious Sun, which would have occupied the position E, obtained by producing the Sun's path backwards for a time T. Does any inconvenience arise from the use of this fictitious Sun, as from that of the fictitious Earth? Only if the velocity of the actual Sun changes; for then the fictitious Sun will sweep out a huge orbit in space, just as the fictitious Earth did.

22. Now we do not yet know whether the velocity of our Sun in space has changed during the period of astronomical observations, but there is a possibility that we may be in a position to detect such a change. A remarkable duplicity of the stellar universe has recently been discovered by Kapteyn, and developed by Eddington. There are some features about the phenomenon which would be more intelligible if the duplicity originated with the Sun rather than with the stars, *e.g.* the curious uniformity in all parts of the sky. Now if the Sun's velocity in space has changed in any way during the period of observation, then the place of any star is virtually referred during the time of observation to a fictitious Sun which is sweeping out a huge orbit in space. If this orbit were the same for all stars, it would be compounded with the actual orbit of the Sun, and we could not separate the two. But the radius of the fictitious Sun's orbit differs for every star, depending on the light-time for each. Hence we should get a duplicity in the parallactic motions. Another way of regarding the matter is this: a change in the Sun's position in space gives a parallactic displacement of the stars, the nearest being most affected. A change in the Sun's velocity will displace the stars in a manner which is in some respects similar to the parallactic, but is independent of the distances of the stars. We might thus conceivably get a duplex effect such as has been observed. On making inquiry of Mr. Eddington whether this was a possibility, I learnt that the same idea had been broached by Mr. Stratton some time ago, and rejected on examination. I hope the details of this examination will be published, as the point is of fundamental importance, and it seems to me still possible that the conclusion is open to revision. Mr. Eddington has kindly promised to go into the matter again.

23. So far, we have considered stellar aberration from the point of view of alternative (α) of § 14. But now let us turn to (β), which has some advantages, especially in offering us positions referred to the time of observation rather than to an epoch in the past—sometimes the very distant past. Our rule is that if we

correct for the whole aberration, *i.e.* if we use the velocity of the Earth relatively to the star, we get the position in which the star *now is*, on a certain hypothesis. The hypothesis is, that it has been moving ever since the light started, with the velocity it then possessed. But since many of the stars conspicuously change their velocity, we find ourselves again in receipt of information cast in an inconvenient form. *Mutatis mutandis*, we may now take fig. 5 to represent the case of a double star. The little orbit $F_3 F_2 F_1$ may represent the orbit of the companion round its primary S, described now in the reverse direction. Suppose light starts at F_3 . Then when it reaches the Earth, if we correct the observation for the full aberration, we get the information that the companion would have been at E_3 if it had persisted ever since with the velocity it had when the light started. If we now make a subsequent observation on light starting from F_2 , we learn that the companion would similarly have now been at E_2 if it had persisted with the velocity of F_2 , and so on. We see how the real orbit is replaced by a fictitious one of much greater dimensions, as was noticed in § 8; but we also see that this larger orbit has no real existence, and conveys to us little of value. The difficulty of § 9 is almost further removed from the domain of reality. We there consider the movement of the surface of a rotating body, which is thereby expanded enormously by aberration. The aberration correction is quite legitimate; but it gives us, *not* an appearance that we actually see (or fail to see), but a totally fictitious body that would have existed if every point of the rotating surface had persisted in a linear path with uniform velocity ever since the light started. Such a body would naturally be huge; but it is a pure mathematical abstraction.

24. Consequently we do not in practice use the fictitious orbit, or the fictitious rotating body. The information that the revolving companion of a double star *would have been* at such and such a point, *if* it had moved with uniform velocity for a long time, does not much interest us; and we seek something which does interest us by correcting for that part of the aberration which depends on its orbital velocity. But we have already applied this correction once, as part of the total correction for relative velocity of Earth and star; and it is easily seen that the second application will be in the opposite direction to the first, and will accordingly annul it. We are thus left with the correction for the velocity of the Earth alone; and thus we arrive at the answer to the question put in § 2, *viz.*—"how is it that we correct for the motion of the Earth in its orbit, and neglect the motion of the star which may also be moving in an orbit?" The answer is that the result will be in an inconvenient form if we use the whole relative velocity, and to get it in a convenient form we must annul the orbital velocity of the star.

25. In conclusion, it seems worthy of remark that there are one or two minute irregularities in the Sun's motion which our present practice (unless I am mistaken) does not take into account, and

which may become sensible in modern refinements. In correcting the places of the stars, the aberration correction is made by the two terms—

$$A = k \cos \omega \cos \odot \quad B = k \sin \odot$$

These depend on the longitude of the Sun relatively to the Earth, including all the perturbations. But they cannot contain any velocities common to Earth and Sun, such as, for instance, the minute rocking of the solar system by Jupiter. As Jupiter revolves, the whole solar system describes a minute orbit round the C.G., and there is a corresponding aberration. The amount is small, the coefficient being about $0''.008$; but such a correction may be sensible in very refined investigations.

26. Again, there is a small term of about the same amount due to the rocking of the Earth by the Moon. I do not think it is included in the N.A. formulæ, although terms of smaller magnitude are retained—(e.g. in D we have the terms

$$+ 0''.0008 \cos 2\Gamma' - 0''.0027 \cos (3\odot - \Gamma). \text{ etc. etc.}).$$

Such terms only come into the observations of stars; planetary observations are fully corrected by the antedating method, if the time of transmission be correctly computed.

Note on the Calculation of Stellar Aberration.

By H. C. Plummer.

The following brief note is suggested by the concluding paragraphs of Professor Turner's paper on Aberration (p. 413), which I have been allowed to see in manuscript. He has pointed out that the usual method of calculating stellar aberration allows only partially for the effect of perturbations of the Earth's orbit. Where great accuracy is required, it may be well, at least as a check, to base the calculation directly on the use of the solar ephemeris.

The rectangular equatorial coordinates of the Sun are (X, Y, Z) . Hence the components of the Earth's velocity relative to the Sun are $(-X', -Y', -Z')$, where X', Y', Z' can be derived from X, Y, Z by mechanical differentiation. Consider the rectangular directions (1) OS of the star; (2) OA perpendicular to OS, and such that the plane SOA contains the pole P; (3) OB perpendicular to both OS and OA. The components of the Earth's velocity vector in these directions are—

$$\begin{aligned} u &= -(X' \cos \alpha \cos \delta + Y' \sin \alpha \cos \delta + Z' \sin \delta). \\ v &= -(-X' \cos \alpha \sin \delta - Y' \sin \alpha \sin \delta + Z' \cos \delta). \\ w &= -(-X' \sin \alpha + Y' \cos \alpha). \end{aligned}$$

The first, u , is the reduction to the Sun of an observed radial

velocity.* The other two can be used for the calculation of the aberration in R.A. and declination. For, if U is the velocity of light, the aberration effects are (in circular measure)

$$\Delta\alpha \cos \delta = w/U, \quad \Delta\delta = v/U.$$

Now the solar ephemeris gives X, Y, Z for 12-hourly intervals. Convenient units of length and time are therefore

$$\begin{aligned} &\text{mean distance of Earth to Sun} \\ &12 \text{ hours} = 43200 \text{ mean solar seconds.} \end{aligned}$$

The ratio of these units divided by U is a number which expresses the light-equation θ in the unit of time chosen. Hence in seconds of arc

$$\Delta\alpha \cos \delta = w\theta/\sin 1'', \quad \Delta\delta = v\theta/\sin 1'',$$

where

$$\theta/\sin 1'' = k T \cos \phi/\pi = [2.06539]k,$$

k being the constant of aberration in seconds of arc, T one sidereal year in mean solar days, and $\sin \phi$ the eccentricity of the Earth's orbit. Hence

$$\Delta\alpha \cos \delta = [2.06539] k (X' \sin \alpha - Y' \cos \alpha).$$

$$\Delta\delta = [2.06539] k (X' \cos \alpha \sin \delta + Y' \sin \alpha \sin \delta - Z' \cos \delta).$$

If we assume $k = 20''.47$ we have

$$[2.06539] k = [3.37650] = 2379''.6.$$

It is to be noticed that the aberration calculated in this way includes the effect of (1) the constant velocity vector perpendicular to the Earth's radius vector, (2) the constant velocity vector perpendicular to the line of apsides, and (3) the disturbance of the mean orbital motion of the Earth by the lunar and planetary perturbations; but it does not include the effect of (4) the motion of the Sun relative to the centre of mass of the solar system, which is very small and due virtually to the planets Jupiter and Saturn alone. And, of course, it is impossible to calculate the effect of that vector, constant or variable, which represents the motion of the solar system as a whole in space. The undisturbed elliptic motion of the Earth is represented by (1) and (2) together, but it is the practice deliberately to ignore the effect of (2), since it merely causes a small constant change in the mean place.† This fact has suggested a point for consideration in connection with photographic planetary observations, which was noticed and discussed by Mr. Cowell,‡ while as regards visual observations the same point had been considered by v. Oppolzer.§

* Cf. Schlesinger, *Astrophys. J.*, ix. p. 159.

† Cf. Bessel, *Tab. Reg.*, p. xix.

‡ *The Observatory*, xxiii. p. 447.

§ *A.N.*, 1560.

In order to remove the effect of the constant velocity parallel to the minor axis, and so to return to the usual practice, it is necessary to apply to $+X'$, $+Y'$, $+Z'$ the following constant corrections—

$$- \cdot 0001414, \quad - \cdot 0000257, \quad - \cdot 0000112.$$

Also the effect of Jupiter in displacing the Sun relatively to the centre of mass of the solar system can be allowed for with sufficient accuracy by applying the corrections

$$- \cdot 0000036 \sin l_J, \quad + \cdot 0000033 \cos l_J, \quad + \cdot 0000014 \cos l_S,$$

while the similar corrections for the action of Saturn are

$$- \cdot 0000008 \sin l_S, \quad + \cdot 0000007 \cos l_S, \quad + \cdot 0000003 \cos l_S,$$

where l_J and l_S represent the heliocentric longitudes of Jupiter and Saturn.

If we compare the general formulæ for the aberration given above with the notation of the *Nautical Almanac* we see that

$$h \sin H = A = - [3 \cdot 37650] Y'.$$

$$h \cos H = B = + [3 \cdot 37650] X'.$$

$$i = A \tan \omega = - [3 \cdot 37650] Z'.$$

Hence the effect of all perturbations will be accounted for if in calculating A , B , and i (for Greenwich mean midnight) we use the formulæ:—

$$\begin{aligned} X' &= \Delta X - \frac{1}{2} \Delta^2 X + \frac{1}{3} \Delta^3 X \\ &\quad - \cdot 0001414 - \cdot 0000036 \sin l_J - \cdot 0000008 \sin l_S \} \\ Y' &= \Delta Y - \frac{1}{2} \Delta^2 Y + \frac{1}{3} \Delta^3 Y \\ &\quad - \cdot 0000257 + \cdot 0000033 \cos l_J + \cdot 0000007 \cos l_S \} \\ Z' &= \Delta Z - \frac{1}{2} \Delta^2 Z + \frac{1}{3} \Delta^3 Z \\ &\quad - \cdot 0000112 + \cdot 0000014 \cos l_J + \cdot 0000003 \cos l_S \} \end{aligned}$$

The coordinate differences are to be formed directly from the 12-hourly solar ephemeris, beginning with the midnight of date.

As an example, let us take the date 1909 May 1, 12^h G.M.T. We find when the corrections have been applied

$$X' = - \cdot 0056393, \quad Y' = + \cdot 0059552, \quad Z' = + \cdot 0025835,$$

which give

$$A = - [1 \cdot 15140], \quad B = - [1 \cdot 12773], \quad i = - [0 \cdot 78871],$$

while the *Nautical Almanac* gives

$$A = - [1 \cdot 15179], \quad B = - [1 \cdot 12762], \quad i = - [0 \cdot 7891].$$

If the former constants be applied to the star Regulus, the resulting effects of aberration are

$$\Delta \alpha. \cos \delta = + 5'' \cdot 850, \quad \Delta \delta = - 2'' \cdot 002,$$

whereas the result of using the *Nautical Almanac* data is to give

$$\Delta\alpha \cdot \cos \delta = + 5''.863, \quad \Delta\delta = - 2''.008.$$

The differences are small, but quite sensible in comparison with some of the terms retained in calculating the nutation. There would thus seem to be a real advantage in calculating those of the day numbers which involve the aberration in the simple manner indicated above.

The question of the signs of the corrections is important, and a little intricate. That they have been rightly assigned may be seen thus. At the beginning of January the Sun passes perigee, and hence X' , Y' , Z' , of which the signs are all positive, are too large numerically. At the beginning of July the Sun passes apogee, and hence X' , Y' , Z' , of which the signs are all negative, are too small numerically. Hence the additive constant corrections required to bring the two sets together numerically must have negative signs.

Again, the position of a planet may be represented by

$$a \cos l, \quad a \sin l \cos i, \quad a \sin l \sin i,$$

and its velocity by

$$-V \sin l, \quad V \cos l \cos i, \quad V \cos l \sin i.$$

The corresponding velocity induced in the Sun will be given by

$$mV \sin l, \quad -mV \cos l \cos i, \quad -mV \cos l \sin i.$$

Now these components are to be added to those of the Earth relative to the Sun, and the latter are $-X'$, $-Y'$, $-Z'$. Hence as additive corrections to X' , Y' , Z' they must be reversed in sign, and thus the signs given are verified.

Note on the observed time of minimum phase of an Algol Variable Star. By H. C. Plummer.

The idea of Dr. A. W. Roberts,* that the relative retardation of the minimum phase of an Algol variable could be used to determine the absolute dimensions of the orbit, was so ingenious that it seemed unfortunate when Father Stein† pointed out that the small interval of time in question must be interpreted differently. Both the dimensions of the orbit and the ratio of the masses are involved, and they cannot be separately determined from the photometric observations.

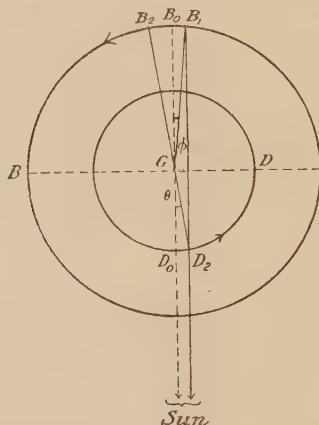
It may be worth pointing out that theoretically a knowledge of the instant of minimum phase, when used in conjunction with a spectrographic determination of the orbit of the bright component, will serve to determine both the dimensions of the orbit of the

* *M.N.*, lxvi, p. 123.

† *M.N.*, lxviii, p. 490.

dark component and the masses of the stars. For the present at least this must be regarded as a theoretical possibility only; an extreme degree of precision is demanded of the photometric and spectrographic observations, and is not yet attainable.

Let G be the centre of gravity of the system (see figure), $B_0 B$ the circular orbit of the bright component, $D_0 D$ the orbit of the dark component. Let b and d be the radii of these orbits. Let B_0, D_0 be the positions of the stars when they are in line with the Sun. Now the minimum phase will occur when the light starts



from B_1 and is occulted by the dark body on reaching D_2 , where $B_1 D_2$ is parallel to $B_0 D_0$. During the passage of the light from B_1 to D_2 the bright star will have moved on to the position B_2 . Let $B_0 G B_1 = \phi$, and $B_0 G B_2 = D_0 G D_2 = \theta$. Since $B_1 D_2$ is parallel to $B_0 D_0$,

$$d \sin \theta = b \sin \phi. \quad (1)$$

and since the light-time from B_1 to D_2 is equal to the orbital time from B_1 to B_2 ,

$$\frac{d \cos \theta + b \cos \phi}{U} = \frac{\theta + \phi}{n} \quad (2)$$

where U is the velocity of light and n is the orbital mean motion. Hence θ and ϕ are determined in terms of b and d .

The observed phenomenon starts from D_2 at a time θ/n after the dark body left D_0 . Let BD be perpendicular to $B_0 D_0$. The observed times are to be reduced to a constant distance by referring the phenomena to the line BD . Hence the actual retardation of the minimum phase is

$$R = \theta/n - d \cos \theta/U \quad (3)$$

But again, the radial velocity curve determined by spectrographic observations will give, *inter alia*, the instant when the

radial velocity of the bright body is halfway between its minimum and its maximum amount, and this velocity corresponds to the body passing through the position B_0 . The retardation in this observed time is clearly

$$R' = b/U,$$

so that the phenomenon now considered will follow the time when the minimum phase is observed at an interval

$$R' - R = b/U + d \cos \theta/U - \theta/n \quad . \quad . \quad (4)$$

Since the orbital velocities are small compared with the velocity of light, θ and ϕ are small angles. Neglecting powers of θ and ϕ above the first, we obtain from (1) and (2)

$$\theta/b = \phi/d = n/U,$$

and hence

$$R = (b - d)/U \quad . \quad . \quad . \quad (5)$$

$$R' - R = d/U \quad . \quad . \quad . \quad (6)$$

The retardation R can be determined from the quadrant points of the light-curve, as Dr. A. W. Roberts pointed out, when both components contribute to the observed light. The observed interval $R' - R$, which can be determined whether the companion be luminous or dark, will give d , and b and d with n will give the masses of the two stars. The motion of the system as a whole relative to the Sun has been neglected. The effect of a uniform radial velocity of G , the centre of gravity, will clearly be to alter the time-scale to which the observed phenomena are referred, and unless allowed for, will cause a small error in the calculation of the masses. Of course the bearing of such a remark is purely theoretical.

The spectrographic determination of the orbit of the bright component will give the radius b , but it will by itself give no information as to d . It has been assumed that the plane of the orbit contains the line of sight, and that the luminosity of the darker component is negligible. If the lines of both components appear in the spectrum, $b \sin i$ and $d \sin i$ can be separately determined, i being the inclination of the orbit. Thus the ratio of the masses will be found, but i can only be found from the light-curve. An excellent example of this type of star is V Puppis, which Professor Pickering* announced to be a spectroscopic binary with composite spectrum, and which has been investigated as a light-variable by Dr. Roberts.† The periods found for the two variable characters appear to be totally different, and it is of urgent importance that a spectrographic investigation of the orbit of this star should be made with the greatest possible thoroughness.

So far as actual observations of Algol are concerned, it is evident that the theory of the present note cannot be applied.

* *H.C.O. Circular*, No. 14. † *Astroph. J.*, xiii. p. 181.

For, so far from finding that the time of "mid-velocity" follows the time of light-minimum by a very short interval, Dr. Schlesinger * finds the observations of Belopolsky, Curtiss, and himself to agree at least in this, that the minimum phase lags an hour or two behind the time demanded by the velocity determinations. This suggests that the influence of tidal and rotational forces is operating to modify the figures of the stars; at any rate, the effect we have to seek is evidently masked by disturbing causes, such as have been suggested by Professor Lebedew † in a recent article.

The eccentricity of the orbit has been neglected. But in any case the star B has its mid-velocity (which must not be confused with the mean velocity, or velocity of the system) when it comes to B_0 where B_0G is perpendicular to the line of nodes BD . Hence neither eccentricity nor inclination will affect the qualitative result and allowance for the effects of these factors will be made with sufficient accuracy by interpreting b and d as the projections on the plane containing the line of sight and the line of nodes of the actual radii vectores at B_0 and D_0 . For the mean motion n must also be substituted the actual angular motion about G at the same points.

Note on Mr. W. S. Franks' Analysis of the Colours and Magnitudes of 3630 Stars (M.N., lxi. p. 106). By Julia Bell, M.A.

(Communicated by Professor Karl Pearson, F.R.S.)

Mr. Franks' tables admit at once of treatment by the method of contingency. We have the following results: if C_2 be the coefficient of mean square contingency,

Combined galactic regions,	$C_2 = \cdot 22$
Combined non-galactic regions,	$C_2 = \cdot 18$
Total stars of both series,	$C_2 = \cdot 19$

It would appear that the relation between luminosity and colour is slightly more intense in the galactic regions.

Previous values for the contingency of magnitude and colour (Gibson and Pearson, *Monthly Notices*, lxviii. p. 419) are $C_2 = \cdot 30$ and $\cdot 27$. These values are distinctly higher, but it may be noted that they were for *photographic* magnitudes, while Mr. Franks' results are for *visual* magnitudes, and contain no blue or green stars.

The percentages values of the different colours in the two groups are compared with those of stars in general by Mr. Franks on p. 107. It is interesting to note the probability that the galactic or the non-galactic stars are a "random sample" from stars in general. Applying the usual method for "goodness of fit"

* *Publ. Allegheny Obs.*, i., No. 5. † *Astrophys. J.*, xxix. p. 101.

(Pearson, *Phil. Mag.*, 2, p. 157, "on the criterion that a given system of deviations from the probable . . . is such that it can be reasonably supposed to have arisen from random sampling"), we learn that only once in 500 samples would there arise a group so divergent as the stars of the galactic region from stars as a whole, and only once in 2500 samples would there arise a group so divergent as the non-galactic stars from stars as a whole. It is therefore unlikely that these groups are random samples. But the differentiation is not as great as one frequently finds in investigations of this kind. The two groups of stars are significantly different, but the difference* is not of that sweeping character which occurs, say, in the deviations between theory and observation in the colour runs of some roulette tables, where the differentiation is such that not one sample in 10^9 trials would deviate as much as observation does. It seemed worth while to reduce Mr. Franks' data to an estimate of this kind, because, while some stress must be laid on the colour differentiation between galactic and non-galactic stars, it is still not one of the kind which produces a sweeping conviction that it is of fundamental importance.

On the Orbit of Jupiter's Eighth Satellite. By P. H. Cowell, M.A., F.R.S., A. C. D. Crommelin, B.A., and C. Davidson. (Plate 16.)

In the *Monthly Notices* of last June an orbit was given for Jupiter's Eighth Satellite based upon the observations made within three months. Having been given the observed right ascensions and declinations, the problem was to deduce the geocentric distance and the motion in the line of sight. This was done with sufficient accuracy to assign the satellite's position for the next opposition to within four minutes of arc. Nevertheless, the error of geocentric distance was of such a magnitude that, if placed transverse to the line of sight, it would have subtended about one minute of arc. For an accurate orbit such an error is inadmissible; and hence it follows that the observations at any one opposition can only be considered as giving rise to four equations of condition, viz. observed right ascension and declination, and observed motion in these coordinates. The material at our disposal now amounts to eight observed quantities, viz. the above four at the oppositions of 1908 and 1909. When the conditions of the problem are assumed, there are only six arbitrary constants in the motion. An orbit, however, that satisfies the observed right ascension and declination in 1908 February, 1908 March, and 1909 January will be sure to satisfy the observed place in 1909 February, unless the

* It does not exceed the divergence given by ordinary dice, which are usually loaded, though not intentionally.

assumed conditions were in more serious error than is conceivable. We have assumed that the Sun supplies the sole disturbing force, and that its mass is 1047.35 times that of Jupiter, and that its position may be taken from the *Connaissance des Temps*. Before long, Jupiter's Eighth Satellite will afford the best determination of the mass of Jupiter, but the time for that has clearly not yet come.

We found it convenient to use equatorial coordinates for 1910.0 instead of ecliptic coordinates for 1908.0. We began, therefore, by transforming the orbit given in *Monthly Notices*, vol. lxviii. p. 576, to these new axes of reference, and we carried that orbit forward to 1909, with the result that a discordance of four minutes was disclosed by the first observation of 1909. We then investigated separately the effects of six independent variations of the initial conditions, with a view to determining the modification which was required. Each successive approximation diminished the error in 1909, and finally we determined the orbit exhibited in Table II., the errors of which do not exceed three seconds of arc. Until observations have been obtained in 1910, we do not think it necessary to go further. Greater accuracy will then be required, for the cumulative error of our calculation may well amount to one second of arc.

Table I. gives the Jovicentric coordinates of the Sun referred to the mean equator and equinox of 1910.0, and also the reversed attractions of the Sun on Jupiter.

Table II. gives the Jovicentric coordinates of the satellite referred to the same axes. A uniform interval of 16 days has been used in copying out for press, but 8-day intervals were used in the calculations from 1908 July 6 to 1909 January 30, the period during which the radius vector was less than 0.14.

In addition, it is only necessary to state that we have adopted

$$\log \frac{\text{mass of Sun} \times \text{square of 16 days}}{\text{cube of astronomical unit of length}} = 8.8794028.$$

$$\log \frac{\text{mass of Jupiter} \times \text{square of 16 days}}{\text{cube of astronomical unit of length}} = 5.8593110.$$

Table III. gives the coordinates of Table II. reduced to geocentric right ascension and declination, and interpolated to four-day intervals for the oppositions of 1908, 1909, and 1910.

Table IV. gives a comparison of tabular minus observed positions: the observed places of 1909 are not definitive. Better places will ultimately be obtained in the manner described in the *M.N.*, lxviii. p. 582.

TABLE I.

Jovicentric Coordinates of Sun and reversed Attractions of Sun on Jupiter in Eighth Decimal Place.

Paris, Noon. 1908.	Jovicentric Coordinates of Sun.			Reversed Attractions of Sun on Jupiter in Eighth Decimal Place		
	x.	y.	z.	X.	Y.	Z.
Jan. 28	+3'301 953	-3'782 108	-1'704 680	-1 678 07	+1 922 08	+866 33
Feb. 13	3'396 903	3'716 341	1'678 776	1 721 24	1 883 10	850 65
29	3'490 135	3'648 687	1'652 017	1 763 35	1 843 45	834 66
Mar. 16	3'581 600	3'579 188	1'624 422	1 804 37	1 803 15	818 37
Apr. 1	3'671 260	3'507 884	1'596 009	1 844 31	1 762 23	801 78
17	3'759 077	3'434 815	1'566 791	1 883 16	1 720 71	784 90
May 3	3'845 009	3'360 023	1'536 791	1 920 91	1 678 62	767 76
19	3'929 008	3'283 556	1'506 022	1 957 57	1 635 98	750 35
June 4	4'011 057	3'205 446	1'474 509	1 993 12	1 592 81	732 69
20	4'091 111	3'125 748	1'442 247	2 027 58	1 549 14	714 79
July 6	4'169 135	3'044 499	1'409 279	2 060 93	1 504 99	696 65
22	4'245 100	2'961 736	1'375 607	2 093 19	1 460 38	678 29
Aug. 7	4'318 963	2'877 513	1'341 266	2 124 33	1 415 34	659 72
23	4'390 703	2'791 876	1'306 263	2 154 37	1 369 88	640 94
Sept. 8	4'460 283	2'704 872	1'270 616	2 183 30	1 324 03	621 96
24	4'527 681	2'616 543	1'234 344	2 211 12	1 277 81	602 80
Oct. 10	4'592 865	2'526 932	1'197 465	2 237 85	1 231 23	583 46
26	4'655 810	2'436 086	1'160'012	2 263 47	1 184 33	563 95
Nov. 11	4'716 494	2'344 053	1'121 989	2 287 98	1 137 11	544 28
27	4'774 881	2'250 885	1'083 424	2 311 40	1 089 60	524 46
Dec. 13	4'830 958	2'156 624	1'044 335	2 333 72	1 041 81	504 49
29	4'884 700	2'061 324	1'004 733	2 354 94	993 77	484 39
1909.						
Jan. 14	4'936 083	1'965 028	0'964 654	2 375 06	945 50	464 16
30	4'985 095	1'867 785	0'924 109	2 394 09	897 00	443 80
Feb. 15	5'031 706	1'769 641	0'883 117	2 412 03	848 31	423 34
Mar. 3	5'075 905	1'670 653	0'841 702	2 428 89	799 43	402 77
19	5'117 670	1'570 860	0'799 884	2 444 66	750 39	382 10
Apr. 4	5'156 989	1'470 324	0'757 686	2 459 35	701 19	361 34
20	5'193 847	1'369 078	0'715 124	2 472 96	651 86	340 49
May 6	5'228 234	1'267 180	0'672 221	2 485 50	602 42	319 57
22	5'260 128	1'164 680	0'628 999	2 496 96	552 87	298 58
June 7	5'289 525	1'061 629	0'585 476	2 507 35	503 24	277 53
23	+5'316 411	-0'958 068	-0'541 678	-2 516 68	+453 53	+256 42

TABLE I.—*continued.*

Paris, Noon. 1909.	Jovicentric Coordinates of Sun.			Reversed Attractions of Sun on Jupiter in Eighth Decimal Place.		
	<i>x.</i>	<i>y.</i>	<i>z.</i>	<i>X.</i>	<i>Y.</i>	<i>Z.</i>
July 9	+5'340 781	-0'854 062	-0'497 624	-2 524 95	+403 77	+235 26
25	5'362 621	0'749 646	0'453 330	2 532 16	353 97	214 06
Aug. 10	5'381 929	0'644 875	0'408 827	2 538 32	304 15	192 82
26	5'398 693	0'539 805	0'364 127	2 543 42	254 31	171 55
Sept. 11	5'412 914	0'434 476	0'319 259	2 547 47	204 48	150 25
27	5'424 585	0'328 942	0'274 237	2 550 48	154 66	128 94
Oct. 13	5'433 702	0'223 255	0'229 088	2 552 45	104 87	107 61
29	5'440 270	0'117 466	0'183 832	2 553 39	55 13	86 28
Nov. 14	5'444 275	-0'011 615	0'138 489	2 553 28	+ 5 45	64 95
30	5'445 727	+0'094 234	0'093 080	2 552 15	- 44 16	43 62
Dec. 16	5'444 627	0'200 041	0'047 629	2 549 99	93 69	22 31
1910.						
Jan. 1	5'440 972	0'305 757	-0'002 155	2 546 80	143 12	+ 1 01
17	5'434 771	0'411 331	+0'043 322	2 542 60	192 44	- 20 27
Feb. 2	5'426 023	0'516 709	0'088 780	2 537 38	241 63	41 52
18	5'414 732	0'621 853	0'134 194	2 531 15	290 69	62 73
Mar. 6	5'400 915	0'726 702	0'179 546	2 523 91	339 60	83 90
22	5'384 565	0'831 210	0'224 814	2 515 66	388 34	105 03
Apr. 7	5'365 699	0'935 328	0'269 977	2 506 40	436 91	126 11
23	5'344 326	1'039 014	0'315 013	2 496 15	485 29	147 13
May 9	5'320 454	1'142 204	0'359 901	2 484 90	533 46	168 09
25	+5'294 093	+1'244 873	+0'404 624	-2 472 66	-581 43	-188 98

TABLE II.

*Jovicentric Coordinates of Jupiter's Eighth Satellite
to Seven Decimal Places.*

Paris, Noon. 1908.	<i>x.</i>	<i>y.</i>	<i>z.</i>
Jan. 28	+ '112 0131	- '123 6476	- '111 5381
Feb. 13	+ '098 2099	- '132 0784	- '106 1080
29	+ '083 6040	- '139 3770	- '099 6940
Mar. 16	+ '068 2740	- '145 3440	- '092 2760
Apr. 1	+ '052 3172	- '149 7601	- '083 8415
17	+ '035 8551	- '152 3850	- '074 3889
May 3	+ '019 0409	- '152 9563	- '063 9318
19	+ '002 0693	- '151 1891	- '052 5055
June 4	- '014 8100	- '146 7772	- '040 1764
20	- '031 2737	- '139 3980	- '027 0554

TABLE II.—*continued.*

Paris, Noon. 1908.		<i>x.</i>	<i>y.</i>	<i>z.</i>
July	6	- '046 9013	- '128 7255	- '013 3174
	22	- '061 1462	- '114 4571	+ '000 7707
Aug.	7	- '073 3070	- '096 3681	+ '014 8137
	23	- '082 5134	- '074 4091	+ '028 2468
Sept.	8	- '087 7618	- '048 8581	+ '040 3079
	24	- '088 0547	- '020 5104	+ '050 0665
Oct.	10	- '082 6833	+ '009 1957	+ '056 5661
	26	- '071 5793	+ '038 2946	+ '059 0944
Nov.	11	- '055 4964	+ '064 7421	+ '057 4528
	27	- '035 8195	+ '086 9761	+ '052 0212
Dec.	13	- '014 1195	+ '104 2112	+ '043 5663
	29	+ '008 2309	+ '116 3588	+ '032 9638
1909.				
Jan.	14	+ '030 2153	+ '123 7656	+ '021 0049
	30	+ '051 1624	+ '126 9693	+ '008 3227
Feb.	15	+ '070 6654	+ '126 5483	- '004 6077
Mar.	3	+ '088 4990	+ '123 0481	- '017 4401
	19	+ '104 5554	+ '116 9555	- '029 9249
Apr.	4	+ '118 8008	+ '108 6948	- '041 8823
	20	+ '131 2466	+ '098 6336	- '053 1821
May	6	+ '141 9318	+ '087 0914	- '063 7295
	22	+ '150 9113	+ '074 3479	- '073 4556
June	7	+ '158 2492	+ '060 6505	- '082 3102
	23	+ '164 0141	+ '046 2198	- '090 2575
July	9	+ '168 2765	+ '031 2546	- '097 2724
	25	+ '171 1064	+ '015 9355	- '103 3385
Aug.	10	+ '172 5724	+ '000 4277	- '108 4463
	26	+ '172 7408	- '015 1169	- '112 5919
Sept.	11	+ '171 6746	- '030 5577	- '115 7757
	27	+ '169 4337	- '045 7639	- '118 0023
Oct.	13	+ '166 0739	- '060 6130	- '119 2789
	29	+ '161 6474	- '074 9898	- '119 6155
Nov.	14	+ '156 2023	- '088 7851	- '119 0238
	30	+ '149 7830	- '101 8943	- '117 5172
Dec.	16	+ '142 4306	- '114 2160	- '115 1105
1910.				
Jan.	1	+ '134 1831	- '125 6506	- '111 8194
	17	+ '125 0762	- '136 0995	- '107 6605

TABLE II.—*continued.*

Paris, Noon. 1908.	x.	y.	z.
Feb. 2	+ '115 1440	- '145 4625	- '102 6514
18	+ '104 4200	- '153 6363	- '096 8105
Mar. 6	+ '092 9387	- '160 5122	- '090 1578
22	+ '080 7376	- '165 9736	- '082 7151
Apr. 7	+ '067 8594	- '169 8929	- '074 5072
23	+ '054 3558	- '172 1283	- '065 5630
May 9	+ '040 2915	- '172 5195	- '055 9181
25	+ '025 751	- '170 883	- '045 618

TABLE III.

*Ephemeris of Jupiter's Eighth Satellite (VIII - 4).**Paris Noon.*

1908.	R.A.	Diff.	Dec.	Diff.
Jan. 28	m s - 0 7'22	s + 8'48	' '' - 43 9'4	' '' + 1 17'0
Feb. 1	+ 1'26	+ 8'52	- 41 52'4	+ 1 21'6
5	+ 9'78	+ 8'61	- 40 30'8	+ 1 25'6
9	+ 18'39	+ 8'73	- 39 5'2	+ 1 28'8
13	+ 27'12	+ 8'87	- 37 36'4	+ 1 31'4
17	+ 35'99	+ 9'04	- 36 5'0	+ 1 33'2
21	+ 45'03	+ 9'20	- 34 31'8	+ 1 34'4
25	+ 54'23	+ 9'38	- 32 57'4	+ 1 34'9
29	+ 63'61	+ 9'55	- 31 22'5	+ 1 34'9
Mar. 4	+ 73'16	+ 9'70	- 29 47'6	+ 1 34'2
8	+ 82'86	+ 9'85	- 28 13'4	+ 1 33'0
12	+ 92'71	+ 9'97	- 26 40'4	+ 1 31'5
16	+ 1 42'68	+ 10'07	- 25 8'9	+ 1 29'5
20	+ 1 52'75	+ 10'15	- 23 39'4	+ 1 27'1
24	+ 2 2'90	+ 10'19	- 22 12'3	+ 1 24'5
28	+ 2 13'09	+ 10'19	- 20 47'8	+ 1 21'8
Apr. 1	+ 2 23'28	+ 10'16	- 19 26'0	+ 1 18'8
5	+ 2 33'44	+ 10'10	- 18 7'2	+ 1 15'7
9	+ 2 43'54	+ 9'99	- 16 51'5	+ 1 12'5

TABLE III.—*continued.*

	R.A.	Diff.	Dec.	Diff.
1908.	m s	s	' "	' "
Apr. 13	+ 2 53'53	+ 9'84	- 15 39'0	+ 1 9'4
17	+ 3 3'37	+ 9'66	- 14 29'6	+ 1 6'2
21	+ 3 13'03	+ 9'44	- 13 23'4	+ 1 3'1
25	+ 3 22'47	+ 9'19	- 12 20'3	+ 1 0'0
29	+ 3 31'66	+ 8'90	- 11 20'3	+ 0 6'9
May 3	+ 3 40'56		- 10 23'4	

*Ephemeris of Jupiter's Eighth Satellite (VIII - 2).**Paris Noon.*

	R.A.	Diff.	Dec.	Diff.
1909.	m s	s	' "	' "
Jan. 2	- 5 35'50	- 14'12	+ 19 29'9	- 1 20'2
6	- 5 49'01	- 13'51	+ 18 3'9	- 1 26'0
10	- 6 1'88	- 12'87	+ 16 32'1	- 1 31'8
14	- 6 14'09	- 12'21	+ 14 55'1	- 1 37'0
18	- 6 25'64	- 11'55	+ 13 13'2	- 1 41'9
22	- 6 36'49	- 10'85	+ 11 26'9	- 1 46'3
26	- 6 46'62	- 10'13	+ 9 36'8	- 1 50'1
30	- 6 56'00	- 9'38	+ 7 43'4	- 1 53'4
Feb. 3	- 7 4'63	- 8'63	+ 5 47'3	- 1 56'1
7	- 7 12'47	- 7'84	+ 3 49'2	- 1 58'1
11	- 7 19'49	- 7'02	+ 1 49'6	- 1 59'6
15	- 7 25'66	- 6'17	- 0 10'7	- 2 0'3
19	- 7 30'96	- 5'30	- 2 11'2	- 2 0'5
23	- 7 35'38	- 4'42	- 4 11'2	- 2 0'0
27	- 7 38'89	- 3'51	- 6 10'0	- 1 58'8
Mar. 3	- 7 41'48	- 2'59	- 8 7'2	- 1 57'2
7	- 7 43'13	- 1'65	- 10 2'1	- 1 54'9
11	- 7 43'86	- 0'73	- 11 54'2	- 1 52'1
15	- 7 43'65	+ 0'21	- 13 43'1	- 1 48'9
19	- 7 42'53	+ 1'12	- 15 28'4	- 1 45'3
23	- 7 40'49	+ 2 04	- 17 9'7	- 1 41'3
		+ 2'92		- 1 37'3

TABLE III.—*continued.*

	R.A.		Diff.	Dec.	Diff.
1909.	m	s	s	' "	' "
Mar. 27	- 7	37'57	+ 3'78	- 18 47'0	- 1 33'0
31	- 7	33'79	+ 4'59	- 20 20'0	- 1 28'7
Apr. 4	- 7	29'20	+ 5'38	- 21 48'7	- 1 24'2
8	- 7	23'82	+ 6'12	- 23 12'9	- 1 20'0
12	- 7	17'70	+ 6'83	- 24 32'9	- 1 15'7
16	- 7	10'87	+ 7'50	- 25 48'6	- 1 11'8
20	- 7	3'37	+ 8'12	- 27 0'4	- 1 7'9
24	- 6	55'25	+ 8'70	- 28 8'3	- 1 4'3
28	- 6	46'55	+ 9'24	- 29 12'6	- 1 1'0
May 2	- 6	37'31	+ 9'73	- 30 13'6	- 0 58'1
6	- 6	27'58	+ 10'19	- 31 11'7	- 0 55'4
10	- 6	17'39	+ 10'60	- 32 7'1	- 0 53'0
14	- 6	6'79	+ 10'99	- 33 0'1	- 0 51'1
18	- 5	55'80	+ 11'33	- 33 51'2	- 0 49'3
22	- 5	44'47		- 34 40'5	

*Ephemeris of Jupiter's Eighth Satellite (VIII - 4).**Paris Noon.*

	R.A.		Diff.	Dec.	Diff.
1910.	m	s	s	' "	' "
Jan. 17	+ 7	20'38	+ 10'73	- 77 7'5	+ 2'1
21	+ 7	31'11	+ 10'41	- 77 5'4	+ 6'4
25	+ 7	41'52	+ 10'07	- 76 59'0	+ 10'7
29	+ 7	51'59	+ 9'70	- 76 48'3	+ 15'4
Feb. 2	+ 8	1'29	+ 9'29	- 76 32'9	+ 20'3
6	+ 8	10'58	+ 8'84	- 76 12'6	+ 25'4
10	+ 8	19'42	+ 8'35	- 75 47'2	+ 31'0
14	+ 8	27'77	+ 7'81	- 75 16'2	+ 36'7
18	+ 8	35'58	+ 7'23	- 74 39'5	+ 42'8
22	+ 8	42'81	+ 6'61	- 73 56'7	+ 49'1
26	+ 8	49'42	+ 5'95	- 73 7'6	+ 55'7
Mar. 2	+ 8	55'37	+ 5'25	- 72 11'9	+ 1 2'3

TABLE III.—*continued.*

1910.		R.A.		Diff. s	Dec.		Diff.
		m	s		'	"	' "
Mar.	6	+	9 0'62		- 71	9'6	' "
	10	+	9 5'13	+ 4'51	- 70	0'4	+1 9'2
	14	+	9 8'88	+ 3'75	- 68	44'4	+1 16'0
	18	+	9 11'84	+ 2'96	- 67	21'6	+1 22'8
	22	+	9 14'00	+ 2'16	- 65	52'1	+1 29'5
	26	+	9 15'34	+ 1'34	- 64	16'0	+1 36'1
	30	+	9 15'86	+ '52	- 62	33'7	+1 42'3
Apr.	3	+	9 15'56	- '30	- 60	45'5	+1 48'2
	7	+	9 14'46	- 1'10	- 58	52'0	+1 53'5
	11	+	9 12'56	- 1'90	- 56	53'4	+1 58'6
	15	+	9 9'91	- 2'65	- 54	50'5	+2 2'9
	19	+	9 6'52	- 3'39	- 52	43'7	+2 6'8
	23	+	9 2'44	- 4'08	- 50	33'7	+2 10'0
	27	+	8 57'69	- 4'75	- 48	21'1	+2 12'6
May	1	+	8 52'31	- 5'38	- 46	6'4	+2 14'7
	5	+	8 46'36	- 5'95	- 43	50'2	+2 16'2
	9	+	8 39'86	- 6'50	- 41	33'0	+2 17'2
	13	+	8 32'86	- 7'00	- 39	15'3	+2 17'7
	17	+	8 25'37	- 7'49	- 36	57'5	+2 17'8
	21	+	8 17'45	- 7'92	- 34	40'0	+2 17'5
	25	+	8 9'08	- 8'37	- 32	22'9	+2 17'1

TABLE IV.

Observed Positions of Jupiter's Eighth Satellite.

1908.

Date & G.M.T.	Aberra- tion Time.	Apparent R.A.		Apparent Dec.			Sat. VIII. - Jupiter.		Tab. - Obs.	
		h	m	s	'	"	R.A.	Dec.	R.A.	Dec.
1908. d h m	m	h	m	s			m	s		
Jan. 27 12 41	35'8	8 45	51'86		+ 18	5' 1'4	- 0	8'40	- 43	18'1
Feb. 1 11 52	35'8	8 43	20'67		18	17 36'3	+ 0	2'08	41	41'8
" 3 10 27	35'9	8 42	21'90		18	22 25'7	0	6'26	41	4'9
" 3 11 28	35'9	8 42	20'52		18	22 34'7	0	6'26	41	1'3
" 22 10 57	36'6	8 33	35'41		19	5 49'8	0	48'27	33	58'9
" 23 8 34	36'6	8 33	13'98		19	7 37'6	0	50'20	33	36'4
" 24 12 25	36'6	8 32	47'37		19	9 51'3	0	53'04	33	9'9

TABLE IV.—*continued.*

Date & G.M.T.	Aberra- tion Time.	Apparent R.A.	Apparent Dec.	Sat. VIII. - Jupiter.		Tab. - Obs.	
				R.A.	Dec.	R.A.	Dec.
1908. d h m	m	h m s	° ' " "	m s	' " "	s	" "
Feb. 27 10 59	36.9	8 31 42.89	19 15' 18".1	0 59.83	32 0.4	+0.11	+0.9
„ 28 11 29	37.0	8 31 21.76	19 17 6.7	1 2.23	31 35.9	+0.12	+0.7
Mar. 3 9 44	37.3	8 30 6.62	19 23 36.5	1 11.72	30 4.6	-0.01	+2.8
„ 8 20 22	37.8	8 28 39.71	19 31 33.3	1 24.72	27 54.4	+0.19	+0.6
„ 23 7 36	39.3	8 26 37.40	19 45 44.0	2 1.12	22 27.1	0.00	-0.1
„ 27 8 54	39.7	8 26 32.33	19 47 53.5	2 11.24	21 0.6	+0.22	-0.5
„ 31 9 16	40.3	8 26 40.24	19 49 12.3	2 21.44	19 38.8	+0.25	+0.3
Apr. 1 16 51	40.4	8 26 45.80	19 49 25.8	2 25.08	19 14.0	-0.05	+1.9
„ 3 9 52	40.6	8 26 54.72	19 49 38.8	2 29.22	18 40.9	+0.16	+2.7
„ 24 9 44	43.4	8 31 43.33	19 41 8.9	3 20.92	12 28.8	+0.13	-0.6
„ 29 16 51	44.1	8 33 43.97	+19 35 49.2	+3 33.15	-11 10.2	+0.06	+0.3

The observations on March 3 and 23 were made at Heidelberg; those on March 8, April 1 and 29 at Lick.

The Greenwich observed places given above are definitive.

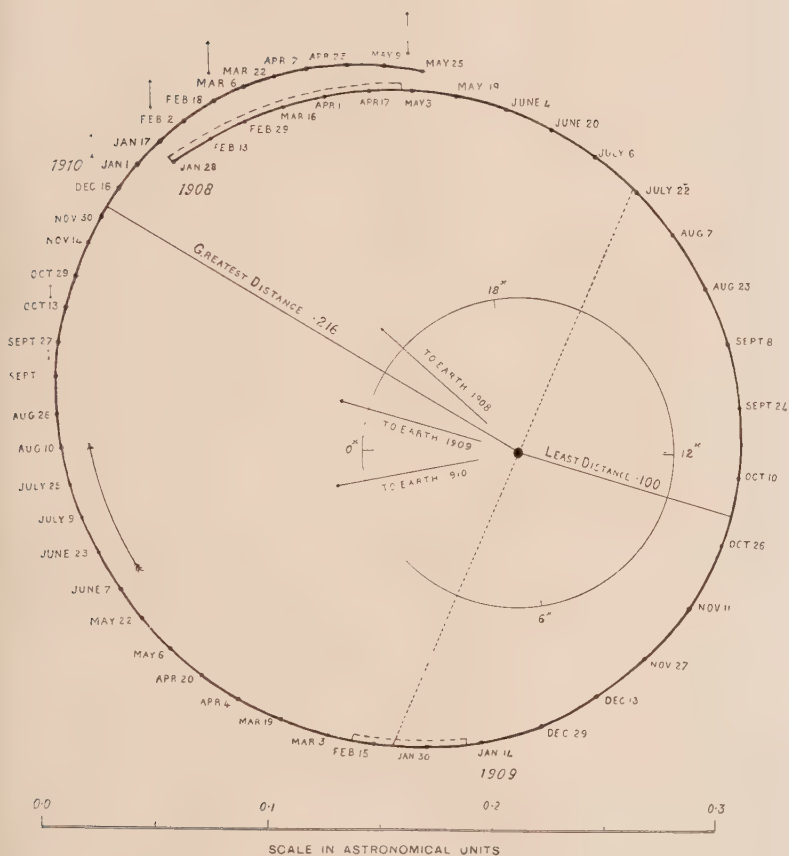
1909.

Date & G.M.T.	Aberra- tion Time.	Apparent R.A.	Apparent Dec.	Sat. VIII. - Jupiter.		Tab. - Obs.	
				R.A.	Dec.	R.A.	Dec.
1909. d h m	m	h m s	° ' " "	m s	' " "	s	" "
Jan. 16 13 15	39.0	10 56 46.68	+7 40 45.6	-6 21.55	+13 53.0	+0.07	-1.8
„ 16 15 3	39.0	10 56 45.48	7 40 49.7	-6 21.85	+13 50.2	+0.15	-0.9
„ 19 13 32	38.7	10 55 59.28	7 44 18.7	-6 29.87	+12 32.9	-0.05	-0.2
Feb. 12 11 35	37.0	10 46 47.87	8 29 41.9	-7 22.01	+1 4.6	+0.17	+0.9
„ 13 11 29	37.0	10 46 19.79	8 32 4.1	-7 23.54	+0 34.0	+0.16	+1.6
„ 15 12 22	36.9	10 45 21.42	8 37 3.8	-7 26.65	-0 24.1	+0.30	-1.5
„ 17 11 26	36.8	10 44 24.50	8 41 54.1	-7 29.22	-1 23.3	+0.22	-1.6
„ 18 10 52	36.8	10 43 55.77	8 44 18.1	-7 30.53	-1 54.8	+0.29	+0.6
„ 20 11 17	36.7	10 42 56.42	8 49 24.4	-7 32.51	-2 53.7	-0.14	-1.1

The observed places given above are not definitive. They will presently be replaced by others reduced with greater refinement.

Although the errors of tabular place amount to three seconds of arc, we have thought it advisable to defer further modification of the orbit until after the observations of 1910 have been reduced.

In order to exhibit the more striking features of the orbit a diagram (Plate 16) has been prepared in which the orbit is projected on a plane from which it does not depart far during 1908. By the middle of 1910, however, the angular distance from this



ORBIT OF JUPITER'S EIGHTH SATELLITE.

plane is fully 5° . This is graphically represented in the diagram by means of the short arrows, the lengths of which indicate the amount the orbit is above the plane of the paper.

It will be seen that rather more than a complete sidereal revolution has been calculated. The curve is not re-entrant, the radius vector being 10 per cent. greater and the angular motion 20 per cent. less in 1910 March than in 1908 March. No exact mean period can for the present be named, but 730 days is probably very near the truth.

The satellite was at least distance ($\cdot 100$ astronomical unit) on 1908 Oct. 19, and at greatest distance ($\cdot 216$ astronomical unit) on 1909 Dec. 7, giving an anomalistic period of 828 days. Owing to evection this may differ considerably from the mean anomalistic month.

The dotted line shows the line of intersection of the orbit plane with the equator, to which it is inclined at an angle of about 35° . From 1908 July 21 to 1909 Feb. 9 the satellite is North of the equator; for the remainder of the time it is South.

The inscribed circle serves to indicate the points 0^h , 6^h , 12^h , and 18^h of Jovicentric R.A., and the direction of the Earth at the times of opposition in 1908, 1909, and 1910.

The dotted arcs show those parts of the orbit actually observed, and on which the whole orbit is based.

Occultations of Planets by the Moon in 1909, visible at British Observatories. By A. M. W. Downing, D.Sc., F.R.S.

The following particulars are computed for the exact positions of the observatories named, but they will also serve as predictions for places not very far distant from the observatories.

It is hoped that astronomers who are favourably placed will not fail to attempt the observation of these phenomena, and will note everything that strikes them in the appearance of the planet under observation, whilst not neglecting to record the times of the various phases of the occultation.

Place.	Planet.	Phase.	Standard Time of Place.			Angle from North Point. Vertex	
			d	h	m	$^{\circ}$	$^{\circ}$
Ottawa	Mars	{ Immersion	Sept. 1	9	1	67	110
		{ Emersion	„ 1	9	59	237	276
Sydney	Venus	{ Immersion	Nov. 17	1	45	160	282
		{ Emersion	„ 17	2	28	208	356

1909 March 8.

*Observations of the Partial Eclipse of the Sun, 1908 December 23,
at the Natal Observatory.*

(Communicated by E. Nevill, F.R.S., Government Astronomer.)

The following observations of the partial eclipse of the Sun were obtained by Mr. A. E. Hodgson, using the 8-inch equatorial refractor in conjunction with the Merz polarising eyepiece and a magnification of 250. The Sun's limb was "boiling" violently during the time of observation.

Observed time of first contact	1 ^h 48 ^m 40 ^s
Computed time of first contact	1 ^h 48 ^m 8
Observed time of last contact	3 ^h 58 ^m 55 ^s
Computed time of last contact	3 ^h 59 ^m 1

The computed times are from the *Nautical Almanac* 1908, p. 453, and the time used is standard time, which is exactly two hours in advance of Greenwich time.

The method of observation was the eye and ear, and the seconds were noted by the beat of a Morse sounder in direct electrical connection with the standard mean time clock, Kullberg 5366.

No micrometer measurements were obtained, as it was considered unsafe for the webs owing to the intense heat, the Sun being almost in the zenith at the commencement of the observations, but measurements of a projected image of the Sun at the time of computed middle eclipse gave the magnitude as '357. The computed value in the *Nautical Almanac* was '367.

On the Data employed in Oppolzer's Canon der Finsternisse.

By E. Nevill, F.R.S.

Mr. Fotheringham is quite correct in supposing that I had misunderstood the real meaning of the empirical corrections to Hansen's Tables employed by Professor Oppolzer in his *Canon der Finsternisse*. Having copies of neither the *Canon* nor the fundamental *Syzygien Tafeln*, the data I had to use were some manuscript extracts taken during my visit to England in 1890, and these gave for the empirical corrections the values

$$\begin{array}{ll}
 \text{Correction to epoch of conjunction} = \Delta T & = +0\cdot006S + 0\cdot009S^2 + 0\cdot00009S^3 \\
 \text{Correction to argument of latitude} = \Delta(g + \omega) & = -0\cdot0190S - 0\cdot040S^2 - 0\cdot00040S^3 \\
 \text{Correction to mean anomaly} = \Delta g & = - \quad + 0\cdot0300S^2 + 0\cdot00300S^3
 \end{array}$$

where S denoted thousands of years from the epoch 1800.

In default of any explanation, these were taken to represent the total corrections applied to each of these quantities, and the last two as being expressed in ordinary degrees of arc.

It was only when I obtained copies of Oppolzer's *Canon und Syzygien Tafeln* some two years ago that I discovered that this was not the case, and learnt that the two last corrections represented explicit extra corrections in addition to those implicitly involved in the correction ΔT ; whilst I discovered, further, that though the correction $\Delta(g + \omega)$ was in sexagesimal degrees, the correction Δg was meant to be in centesimal degrees.

This misunderstanding explains the greater part of the discordance between the results for the ancient eclipses of the Sun obtained by Mr. Cowell and myself, and I have had for some time in hand an unfinished note stating this fact, and giving the amended results of my calculations, but heavy pressure of work has prevented its completion.

But it does not affect the conclusion drawn in my note on these ancient eclipses of the Sun (*M.N.*, vol. lxvi. p. 404). I was concerned to show simply that as it was possible to represent seven or eight selected eclipses out of the known thirty recorded ancient eclipses by many different sets of data, the fact that any given system of data did represent some five or six selected eclipses could not be held to establish the correctness of the assumed system. To prove this, I selected three widely different sets of data, and showed that each served to represent from six to eight of the known ancient eclipses of the Sun.

It is true that, owing to the misunderstanding of Oppolzer's meaning, the actual data corresponding to these calculations were not those specified in the note, but corresponded to approximately secular accelerations of the perigee and node about $3''.7$ and $2''.6$ larger than stated; but this in no way invalidated the conclusion drawn, that the mere fact that any adopted system of data served to represent some seven or eight of the recorded ancient eclipses of the Sun was no proof of the accuracy of the assumed data. The very mistake made only strengthens this conclusion.

At the same time, the results I obtain for the true values of Oppolzer's assumed data do not quite accord with those found by Mr. Fotheringham. I make the data to be

Corrections to Hansen's Tables.

$$\begin{aligned} \text{Correction to mean longitude} &= +0''.00 - 26''.33T - 3''.574T^2 - 3''.574\left(\frac{T}{10}\right)^3 \\ \text{Correction to longitude of Perigee} &= +0''.00 - 0''.24 - 11''.084 - 11''.748 \\ \text{Correction to longitude of Node} &= -5''.50 + 79''.52 + 1''.011 + 0''.922 \end{aligned}$$

corresponding to secular accelerations,

$$\begin{aligned} \text{In mean longitude} &= + 8''.606T^2 = \text{Theory value} + 2''.70T^2 \\ \text{In longitude of Perigee} &= -48''.339 = \quad \quad \quad -9''.44 \\ \text{In longitude of Node} &= + 8''.079 = \quad \quad \quad +1''.52 \end{aligned}$$

These three values are quite inconsistent with theory, unless there is some defect in the values derived by astronomers from the theory of gravitation. It is possible that the values obtained by Professor Newcomb and Professor Brown may not be quite complete, but it seems very doubtful that they could be as much as half a second too small.

Natal Observatory:
1909 February 3.

Note on Mr. Nevill's paper on the Data employed in Oppolzer's Canon der Finsternisse. By P. H. Cowell, M.A., F.R.S.

In the *Monthly Notices*, vol. lxvi. p. 404, Mr. Nevill examines four different sets of formulæ, and on p. 415 he arrives at the conclusion that they represent ancient eclipses about equally well, and that therefore there is no observational ground for preferring one set of formulæ to any other of the four. At that time he believed the fourth set of formulæ to be mine, and he thought that he had shown three other sets to be equally good. Mr. Fotheringham has, however, pointed out that Mr. Nevill's calculations were based on a misconception of Oppolzer's meaning, and Mr. Nevill now admits that the four sets of formulæ that he examined are different from what he supposed, and in particular that the fourth set is not mine. Assuming the accuracy of his arithmetic, he has therefore demonstrated that four sets of formulæ that have nothing to do with me represent ancient eclipses equally well, or perhaps we should say equally badly. Mr. Fotheringham's researches have presented Mr. Nevill with another set of formulæ different from mine, and coming up to a certain standard in satisfying ancient records; and as he has now four sets instead of three (not mine), he considers that his conclusion is strengthened.

It should be noticed, however, that he tacitly assumes that my formulæ do not satisfy the records any better than his four different sets; and as, by his own admission, he has not examined my formulæ at all, I am at a loss to account for this assumption. In the absence of any figures of his own, I beg of him to look at mine, and to notice that, according to my calculations, my formulæ bring the centres of the Sun and Moon to within 50" of each other for the eclipses of - 1062, - 762, - 648, - 602, - 584, - 430, - 309 (besides others after the beginning of the Christian era), and that therefore the residuals according to my formulæ are about one-third as large as the residuals according to any of his four sets.

I can assure him that my conclusions would never have been put forward if my residuals had really been as large as he supposed them to be when he wrote his paper of 1906 May.

On a Chinese Planisphere. By E. B. Knobel.
(Plates 17, 18.)

There was exhibited in the Science Section of the Franco-British Exhibition, by the Royal Scottish Museum, a bronze instrument described in the catalogue as "A Bronze Compass $13\frac{1}{2}$ inches diameter, taken from a Japanese Junk, with raised dots in centre said to represent Stars"; a further label stated that it was believed to represent all the stars visible in Japan.

The instrument is undoubtedly Chinese, and is a planisphere of the heavens, on which the stars are indicated according to the Chinese constellations.

Original Chinese planispheres, whether on metal or paper, are extremely rare. Nothing of the sort is found at the British Museum, though it is possible that some maps of the stars may hereafter be discovered in the collection of Chinese books and manuscripts. The only sources of information known to me are—

(1) *Atlas Celeste Chinoise*, by P. Grimaldi, Soc. Jesu, printed in China, 1711. In these maps the Chinese asterisms are engraved on maps of Tycho Brahe's stars.

(2) The maps of De Guignes in vol. x. of the *Mémoires de l'Académie présentés par divers Savants*, 1785, in which the Chinese asterisms are engraved on the stellar maps of De la Hire.

(3) Williams' work, *Chinese Observations of Comets*, 1871, wherein he gives Chinese stellar maps copied from a Chinese work in the library of University College, London; and

(4) Schlegel's *Uranographie Chinoise*, which is a very exhaustive work on the subject, and must always be considered of the highest value.

All the above, however, are probably from sources subsequent to the Jesuit expedition to China in the 17th. century. In many cases the planisphere in question differs from these texts; and, for the above reasons, I think it may be interesting to offer to the Society some description of the instrument.*

The planisphere or map of the heavens is about $9\frac{1}{2}$ inches diameter. It is a quasi-polar equidistant projection of the sphere, but extended to about 50° south of the equator. The stars are indicated by raised dots, which are linked together according to the usual method with the Chinese, each group of stars so united forming one of their numerous asterisms.

In studying the planisphere it should be remembered that there is no connection whatever between Chinese constellations and those with which we are familiar in Europe, though some well-known groups of stars, like Charles's Wain in Ursa Major, the Pleiades and Hyades in Taurus, and the principal stars in Orion and Corvus form definite Chinese asterisms.

* The Society formerly possessed several Chinese planispheres, as appears from a list published in the 15th volume of the *Monthly Notices*, but these have all disappeared and cannot be found.

The essential difference is that the constellations as we know them are roughly defined areas of the heavens, whereas the Chinese asterisms consist of one or more stars, singly having a special significance, or united together in some definite form, or with some definite idea; in no case are they areas of the heavens. It will be admitted that there are some advantages in this system over the vague constellations which we have inherited from the Greeks. One other point is important, namely, that the relative magnitude of the stars is to a great extent ignored, so that an asterism which may consist of stars of the 1st. to the 6th. magnitude may all be represented as of the same size. It is obvious that this adds very much to the difficulty of identification.

The instrument is shown in the accompanying photograph.*

The centre of the planisphere is the pole of the equator. The projection appears to be equidistant, and at a North Polar distance of 38° is a circle bounding the area of Perpetual Apparition. This indicates that the instrument has been constructed for a latitude a little south of Pekin. The equator is shown, and also the ecliptic; but the obliquity is very incorrect. The Milky Way is also shown in outline fairly correctly.

Radiating from the pole, but not extending into the area of perpetual apparition, are lines at unequal angular distances. These indicate the 28 "Siou," which are the "Domiciles," "Mansions of the Moon," "Stellar Divisions," and "Lunar Zodiac," of which a table is given below.

There are no numbers or names on the planisphere, and therefore all references are to the photographic chart accompanying this paper, which I have made from an enlarged photograph of the instrument. On this I have numbered the Siou, and indicated the principal European constellations by words, and many of the Chinese asterisms by numbers.

The angular intervals of the Siou are measured on the equator, but they are not quite the same as in other authorities, so the table gives also the values from Schlegel's *Uranographie Chinoise*, and from Williams' *Chinese Observations of Comets*. Following Schlegel, Williams, and Wells Williams in his Chinese Dictionary, I have adopted Kio as the first Siou, though Professor W. D. Whitney of Yale College, following Biot, has presented strong arguments in favour of Mao being the first.

* I am indebted to Dr. Dobbie, Director of the Royal Scottish Museum, for his courteous permission to reproduce the photograph of the instrument which he kindly sent me.



CHINESE PLANISPHERE.



Table of Siou.

No.	Name.*	Meaning.	Royal . Scottish Planisphere.	Schlegel.	Williams.
I.	Kio	The horn	12 0	12 0	11
II.	Kang	The neck	9 0	9 0	11
III.	Ti	The base	14 20	16 0	18
IV.	Fang	The house	4 40	5 30	5
V.	Sin	The heart	5 20	6 20	7
VI.	Wi	The tail	17 0	18 50	16
VII.	Ki	The sieve	11 30	10 20	9
VIII.	Teou	The measure	25 30	25 0	24
IX.	Niou	The ox	7 40	7 0	8
X.	Niu	The virgin	11 40	11 10	12
XI.	Hiu	The funeral mound	10 20	8 50	10
XII.	Wei	The ridge	17 0	15 10	20
XIII.	Che	The funeral pyre	15 30	16 50	15
XIV.	Pi	The wall	9 0	8 30	12
XV.	Kouei	The stride †	14 30	16 20	12
XVI.	Leou	The reaper	11 30	11 40	13
XVII.	Wei	The grain store-room	13 30	15 20	13
XVIII.	Mao	The setting sun	11 0	11 0	8
XIX.	Pi	The bird-net	15 30	17 15	15
XX.	Tsoui	The mouth	2 20	0 5	1
XXI.	Tsan	The august	9 0	11 0	11
XXII.	Tsing	The well	32 30	32 50	31
XXIII.	Kouei	The manes	4 0	2 10	4
XXIV.	Liou	The willow	15 0	13 10	17
XXV.	Sing	The asterism	8 0	6 10	9
XXVI.	Tchang	The open net	18 0	17 0	18
XXVII.	Yih	The wings	17 40	18 30	17
XXVIII.	Tchin	The car	17 0	17 0	13
			360 0	360 0	360

On the rim surrounding the planisphere are engraved the names of the twelve Horary Characters or Branches, which indicate the Months, the Chinese hours (each of 120 minutes), and the points of the compass.

* As so much diversity of opinion exists in writing the phonetic value of Chinese words in Roman letters, I have in all cases adopted the transliteration of Schlegel, not because it is the most correct, but for convenience, and to ensure some uniformity.

† The sandal (Schlegel).

Chinese Name.	Months.	Hours.	Points of the Compass.
子 Tsse	December	11 P.M. to 1 A.M.	North
丑 Tcheou	January	1 A.M. ,, 3 A.M.	NNE $\frac{3}{4}$ E
寅 Yin	February	3 ,, ,, 5 ,,	ENE $\frac{3}{4}$ N
卯 Mas	March	5 ,, ,, 7 ,,	East
辰 Tchin	April	7 ,, ,, 9 ,,	ESE $\frac{3}{4}$ E
巳 Sse	May	9 ,, ,, 11 ,,	SSE $\frac{3}{4}$ E
午 Ou	June	11 A.M. ,, 1 P.M.	South
未 Wi	July	1 P.M. ,, 3 ,,	SSW $\frac{3}{4}$ W
申 Chin	August	3 ,, ,, 5 ,,	WSW $\frac{3}{4}$ S
酉 Yeou	September	5 ,, ,, 7 ,,	West
戌 Sou	October	7 ,, ,, 9 ,,	WNW $\frac{3}{4}$ N
亥 Hai	November	9 ,, ,, 11 ,,	NNW $\frac{3}{4}$ W

There are two small compasses on opposite sides of the instrument, and the correct position of the planisphere is with the two compasses north and south of each other; that compass being at the north which has the word 子 Tsse engraved under it. This word stands for the month corresponding to December, and also indicates the winter solstice. The stellar division under it is No. xi., Hiu, containing stars in Aquarius. The winter solstice passed through this division at the epoch of the Emperor Yao B.C. 2350, when the Pleiades marked the vernal equinox. It would appear from this that the 28 Siou on the planisphere have been delineated in relation to the surrounding 12 branches according to the records in the ancient work, the Chou-King, which is believed to date back to the period of the Emperor Yao, B.C. 2350. The equator, however, is for about the epoch A.D. 600. The character of the instrument is modern, and possibly not earlier than the 19th. century.

The Chinese divide the heavens as seen in China into the 28 Siou or Stellar Divisions, and three great stellar spaces. The Stellar Divisions are measured on the equator rather than the ecliptic, and the great inequality in the angular intervals precludes the view that has been advanced by so many writers that they represent the daily stations of the Moon, and no satisfactory explanation has yet been found for this inequality. The subject of the Lunar Zodiac of the Chinese, and its relation to that of the Hindus and of the Arabs, has been extensively discussed by many oriental scholars.* The fact that these nations divide the ecliptic into 28 divisions with determinants in many cases identical, is a convincing argument of a common origin; there is, however, this difference, that whereas the determinants of the Nachshatras of the Hindus and the Manazil of the Arabs, are one or more individual stars, the Chinese adopt determining asterisms for each Siou.

* See the works of J. B. Biot, Sédillot, Weber, Ideler, Schlegel, Williams, and particularly of Prof. W. D. Whitney.

The Siou are numbered on the chart according to the previous table, and the Determining Asterisms are shown by *black rings* to the stars, which are conspicuously linked together. Very little consideration will suffice to show the incompatibility of many of them with the positions of the Moon in the Lunar Zodiac. Noticeable in this connection are the Siou Determining Asterisms 57 (Andromeda), 80 (Orion), 103 (Hydra), 108 (Crater), 111 (Corvus), and 20 (Scorpio), which are far removed from the ecliptic or the Moon's path. Schlegel refutes the idea that there was any connection between the Chinese asterisms and the Lunar Zodiac, and he considers that their origin was purely astrological. It is also remarkable that so conspicuous a constellation as Leo should not have been included in the determinants. The theory was advanced by Biot that these 28 asterisms were selected B.C. 2350 on account of their proximity to the equator. The fact that at this epoch the isolated cluster of the Pleiades marked the vernal equinox offers a plausible argument in support of that epoch being selected, but a reference to the chart accompanying Whitney's memoir on "The Lunar Zodiac,"* wherein these asterisms are delineated in relation to the equator at that epoch, shows too glaring inconsistencies for accepting Biot's view.† The only suggestion that seems to be left is that these asterisms were adopted as points of reference for celestial phenomena, and on this view it is not improbable that the coincidence of the Pleiades with the vernal equinox should have been taken for the epoch of such selection, especially as at that time history records that the Emperor Yao took interest in astronomy, and had certain astronomers attached to his court. This, however, leaves the question of the unequal angular intervals wholly unexplained.

The boundaries of the three great stellar spaces, Tsse-wi-houan (circumpolar region), Tai-wi-houan (part of Leo, Virgo, and Coma), and Tien-chi-houan (part of Hercules, Ophiuchus, and Serpens) are shown on the chart by *black stars*. The names are omitted, to avoid confusion, but they can be easily identified by the numbers attached to the boundary stars.

In the following table of asterisms identified on the planisphere the Chinese names are all taken from Schlegel, and the stars from modern star maps; nothing has been taken from the Chinese authorities mentioned above without verification. Naturally, there are many asterisms which it is impossible to determine with accuracy. In all authorities consulted there is an absence of uniformity in the linking of the stars, which adds much to the difficulty of identification; and particularly is this the case with

* *Oriental and Linguistic Studies*, second series, Cambridge, Mass., 1874.

† The declinations on the planisphere of the stars γ Pegasi, α Orionis, and α Virginis are sufficiently good for A.D. 600 to allow of their correction to B.C. 2350, from which, as the projection is equidistant, a roughly approximate equator for that epoch can be laid down. It is then seen that the positions of the determining asterisms of Siou XIII., XIV., XV. etc., to I. agree fairly with Whitney's chart, but from II. to XII. their positions are much in error.

the circumpolar stars. The pole of the planisphere is one of a chain of five large stars, the most conspicuous in the instrument. The five stars thus linked together are shown in the Chinese Atlas of Grimaldi, but not in De Guignes or Schlegel, and Williams shows only four stars. They consist of γ , β , 5 and 4 Ursæ Minoris, and a sixth magnitude star Bradley 1730, which last is the pole of the planisphere. The name of this star is Tien-tchou, meaning the Pivot of the Heavens. It is not identified by Reeves in his Appendix on Chinese Astronomy to Morrison's Chinese Dictionary, nor by any other authority except Grimaldi. The star Bradley 1730 was near to the pole of the equator at the assumed epoch A.D. 600. It is obvious that the name Tien-tchou could not have always designated this particular star. Schlegel, however, states that the name "Pivots of the Heavens" was also applied to all these five stars. Polaris cannot exactly be identified; it is probably a small star enclosed in the asterism 124.

The Milky Way, Tien-han (the Celestial River), is delineated with approximate correctness, though the bifurcation begins erroneously in Aquila.

Considering the scale of the planisphere, some of the stars and constellations seem to have been laid down with care and skill, while there are large errors in many others, and immense exaggeration in some clusters and groups.

Chinese Asterisms on Planisphere.

No.	Chinese Name.	Stars.
1.	KIO-SIOU.—I.	2 stars, α , ζ , Virginis.
2.	Tien-mun	89, 61, Virginis.
3.	Ping-sing	π , γ , Hydræ.
4.	Ping-tao	74, θ , Virginis.
5.	Tien-tien	σ , τ , Virginis.
6.	Yeou-chi-ti	η , τ , ν , Boötis.
7.	San-koung	21, 24, Groombridge 1991, Can. Ven.
8.	Hiouen-ko	γ Boötis.
9.	Tchao-yao	β Boötis.
10.	Kang-ho	ϵ , σ , ρ , Boötis.
11.	Ti-si	26, 12, 9, Boötis.
12.	Ta-kio	α Boötis.
13.	KANG-SIOU.—II.	4 stars, ϕ , ι , κ , λ , Virginis.
14.	TI-SIOU.—III.	4 stars, β , γ , ι , α , Libræ.
15.	Tso-chi-ti	ξ , π , ζ , Boötis.
15A.	Yeou-chi-ti	η , τ , ν , Boötis.
16.	FANG-SIOU.—IV.	4 stars, β , δ , π , ρ , Scorpii.
17.	Koan-so	Corona Borealis.

Chinese Asterisms on Planisphere—continued.

No.	Chinese Name.	Stars.
18.	Tsi-tsou	Stars in Lupus unidentified.
19.	Tsi-koung	ν , σ , τ , λ , μ , Coronæ; μ , δ , Boötis.
20.	SIN-SIOU.—I.	3 stars, τ , α , σ , Scorpii.
21.	Hoan-tche	60, Bradley 2163, 2 stars unidentified, Herculis.
22.	Ti-tso	α Herculis.
23.	Hao	α Ophiuchi.
24.	WI-SIOU.—VI.	9 stars, ν , λ , κ , ι , θ , η , ζ , μ , ϵ , Scorpii.
25.	Kouei	α , ν , β , ζ , ϵ , Aræ.
26.	Tien-kiang	51, 44, θ , 36, Ophiuchi.
27.	KI-SIOU.—VII.	4 stars, γ , δ , ϵ , η , Sagittarii.
28.	Tien-pi	Corona Australis.
29.	Tien-yo	Stars and clusters in Sagittarius, unidentified.
30.	Tien-ki	μ , κ , Lyræ; 90, θ , ρ , 68, 59, ϵ , ζ , Herculis.
31.	TEOU-SIOU.—VIII.	6 stars, ζ , τ , σ , ϕ , λ , μ , Sagittarii.
32.	Tien-pien	Stars in Aquila unidentified.
33.	Tchi-niu	ϵ , α , ζ , Lyræ.
34.	Ho-kou	β , α , γ , Aquilæ.
35.	Yeou-ki	μ , σ , δ , ν , ι , 42, κ , and 2 stars unidentified, Aquilæ.
36.	NIOU-SIOU.—IX.	6 stars, ν , α , β , ρ , π , σ , Capricorni.
37.	Tien-tien	Unidentified.
38.	Tso-ki	ρ Aquilæ, 14, 15, 13, γ , ζ , δ , α , β , Sagittæ.
39.	Lo-ien	ν , B.A.C. 7077, τ Capricorni.
40.	NIU-SIOU.—X.	4 stars, ϵ , μ , ν , 9, Aquarii.
41.	Tien-tsin	α , ν , σ , ν , ζ , ϵ , γ , δ , σ , Cygni.
42.	HIU-SIOU.—XI.	2 stars, α Equulei, β Aquarii.
43.	Tchai-fou	Stars in Pegasus unidentified.
44.	Jin-sing	„ „
45.	WEI-SIOU.—XII.	3 stars, α Aquarii; θ , ϵ , Pegasi.
46.	Fan-mou	π , η , ζ , γ , Aquarii.
47.	Li-koung	η , σ , Pegasi.
48.	Li-koung	λ , u , Pegasi.
49.	Li-koung	τ , ν , Pegasi.
50.	Loui-pi-tchin	ϵ , κ , γ , δ , Capricorni; ι , σ , λ , ϕ , Aquarii; 27, 29, 33, 30, Piscium.
51.	Tien-tsien	Stars in Piscis Australis unidentified.
52.	CHE-SIOU.—XIII.	2 stars, α , β , Pegasi.
53.	Loui-tien	31, ζ , 30, 55, 70, Pegasi; θ Aquarii?

Chinese Asterisms on Planisphere—continued.

No.	Chinese Name.	Stars.
54.	Pie-lie	ω , ι , θ , γ , β , Piscium.
55.	Tien-kiou	Stars in Andromeda unidentified.
56.	PI-SIOU.—XIV.	2 stars, α Andromedæ, γ Pegasi.
57.	KOUËI-SIOU.—XV.	16 stars, β , μ , ν , B.A.C. 100, ρ , π , δ , ϵ , ζ , η , Andromedæ; ψ , χ , ϕ , υ , 68, σ , Piscium.
58.	Wai-ping	α , ξ , ν , μ , ζ , ϵ , δ , Piscium.
59.	Tien-houan	Stars in Cetus unidentified.
60.	Tou-sse-koung	β Ceti.
61.	Tien-tsang	ν , τ , ζ , θ , η , ι , Ceti.
62.	LEOU-SIOU.—XVI.	3 stars, α , β , γ , Arietis.
63.	Ta-ling	64, θ , Gr. 590, κ , 30, ω , ρ , 16, Persei—stars surrounding β Persei.
64.	Tien-ta-tsiang-kiun	Stars in Triangulum and Perseus unidentified.
65.	WEI-SIOU.—XVII.	3 stars, μ , 39, 41, Arietis.
66.	Yeou-kang	Stars in Aries unidentified.
67.	Tso-kang	„ „
68.	Tien-yuen	π , δ , ϵ , ζ , η , Eridani; ϵ , π , Ceti; τ^1 , τ^2 , τ^3 , τ^4 , τ^5 , τ^6 , Eridani.
69.	Kiouen-chi	ν , ϵ , 50, ξ , ζ , σ , Persei.
70.	MAO-SIOU.—XVIII.	7 stars, Pleiades.
71.	Tien-kou	α Aurigæ.
72.	Wou-tchai	ι , α , β , θ , Aurigæ; β Tauri.
73.	PI-SIOU.—XIX.	8 stars, Hyades (? α , θ , π , γ , δ^1 , δ^3 , ϵ , λ).
74.	Tchu-wang	ζ , 114, 109, ι , τ , χ , 37, Tauri.
	?	ν , τ , υ , Aurigæ.
75.	San-tchou	{ Three triplets of stars in Auriga: ν , τ , υ ; ϕ , χ , 14; ϵ , η , ζ . (near α Aurigæ).
76.	San-tchou	
76A.	San-tchou	
77.	Yo-tsing	τ Orionis; β , ψ , λ , Eridani.
78.	Kiou-yeou	Stars in Eridanus unidentified.
79.	TSOUI-SIOU.—XX.	3 stars, λ , ϕ^1 , ϕ^2 , Orionis.
80.	T'SAN-SIOU.—XXI.	7 stars, α , γ , β , κ , ζ , ϵ , δ , Orionis.
81.	Kiun-tsing	ν , λ , κ , ι , Leporis.
82.	Ping. ? (tsing)	μ , ϵ , Leporis.
83.	Tien-tsse	α , β , γ , δ , Leporis.
84.	TSING-SIOU.—XXII.	8 stars, ϵ , 36, ζ , λ , μ , γ , ξ , Geminorum.
85.	Wou-heou	θ , τ , ι , υ , κ , Geminorum.
86.	Tien-tsoun	δ , 56, 63, Geminorum.
87.	Pe-ho	α , β , ρ , Geminorum.

Chinese Asterisms on Planisphere—continued.

No.	Chinese Name.	Stars.
88.	Nan-ho	α, β, γ , Canis Minoris.
89.	Tien-lang	α Canis Majoris.
90.	Kiun-chi	Stars in Canis Major and Columba unidentified.
91.	Hou-chi	$\rho, 202, o, \eta, \epsilon, \delta, \tau, \xi, o^2$, Argûs.
92.	Lao-jin	α Argûs.
93.	KOUEI-SIOU.—XXIII.	4 stars, $\gamma, \delta, \theta, \eta$, Cancrî.
94.	Chang-tai*	ι, κ , Ursæ Majoris.
95. } 96. }	Hien-youen-ki	31, 35, o , B.A.C. 3097, Lyncis; 64, ι, ξ , Cancrî; $\rho, 31, o, \alpha, \eta, \gamma, \zeta, \mu, \epsilon, \lambda$, Leonis.
97.	LIU-SIOU.—XXIV.	8 stars, $\theta, \omega, \zeta, \epsilon, \delta, \sigma, \eta, \rho$, Hydræ.
98.	Wen-tchang	$o, 5, 23, v, \phi, \theta, 15$, Ursæ Majoris.
99.	Tchoung-tai*	λ, μ , Ursæ Majoris.
100.	SING-SIOU.—XXV.	7 stars, ι, τ^2, τ^1 , B.A.C. 3226, $\alpha, 27, 26$, Hydræ.
101.	Tien-lao	36, 37, 39, 43, 44, 60, Ursæ Majoris.
102.	Hia-tai*	ν, ξ , Ursæ Majoris.
103.	TCHANG-SIOU.—XXVI.	6 stars, $\nu, \mu, \lambda, v^1, \kappa$, one star unidentified, Hydræ.
104.	Tien-miao	Stars in Argo unidentified.
105.	Chi	? χ, ψ , 2 stars unidentified, Ursæ Majoris.
106.	Wou-ti	$\beta, 88$, 3 stars unidentified, Leonis.
107.	Noui-ping	o, π, ν, ξ , Virginis.
108.	YIH-SIOU.—XXVII.	22 stars in Crater, not all identified.
109.	Khi-fou	Stars in Centaurus and Crux, not identified.
110.	Lang-wei	Coma Berenicis.
111.	TCHIN-SIOU.—XXVIII.	7 stars, $\eta, \delta, \gamma, \epsilon, \alpha, \zeta, \beta$, Corvi.
112.	Kou-leou	Stars in Centaurus unidentified.
113.	Nan-mun	α, β , Centauri.
114.	Pe-teou	$\alpha, \beta, \gamma, \delta, \epsilon, \zeta, 81, \eta$, Ursæ Majoris.
115.	Tien-li †	probably Fl. 66, Hev. 51, Fl. 42, Pi. xi. 59, Ursæ Majoris.
116.	Tien-tsiang	θ, ι, κ , Boötis.
117.	Tien-pei	ι Hercules, γ, β, ν, ξ , Draconis.
118.	Tsaou-fou	Stars in Cepheus unidentified.
119.	Tai-tsze	γ Ursæ Minoris.

* 94, 99, and 102 called San-tai.

† All differ as to the position of these four stars in the quadrilateral of Ursa Major. Grimaldi shows undoubtedly H. 66, Pi. xi., 59, and Hev. 51, with another star which may be Fl. 76. Reeves and Williams do not identify, and Schlegel omits them altogether.

Chinese Asterisms on Planisphere—continued.

No.	Chinese Name.	Star.
120.	Tien-ti-sing	β Ursæ Minoris.
120A.	Chou-tsse	5 Ursæ Minoris.
120B.	Hao-fi	4 Ursæ Minoris.
120C.	Tien-tchou	? Bradley 1730, Camelop.
121.	Wang-liang	β , η , μ , α , ζ , Cassiopeiæ.
122.	Ko-tao	ϵ , δ , θ , ξ , Cassiopeiæ ; 22 Andromedæ.
123.	Noui-Kiai	28, 23, τ , 16, 5, σ , Ursæ Majoris.
124.	Kao-tchin	ζ , ϵ , δ , 3 small stars round Polaris, Ursæ Minoris.

The Boundary Stars of the three Stellar Spaces.

TSSE-WI-HOUAN.

This includes Ursæ Minor and circumpolar stars, and consists of two chains of eight and seven stars respectively.

No.	Chinese Name.	Star.
125.	Chang-tching	γ Cephei.
126.	Chaou-wei	ρ "
127.	Chang-wei	κ "
128.	Chao-fou	ϕ Draconis.
129.	Chang-fou	ξ "
130.	Chaou-tsai	η "
131.	Chang-tsai	θ "
132.	Tso-tchou	ι "
133.	Yeou-tchou	α "
134.	Chaou-wei	κ "
135.	Chang-fou	λ "
136.	Chao-fou	Pi. x. 126, Ursæ Majoris.
137.	Chao-wei	24 "
138.	Chang-wei	55 Camelopardi.
139.	Chao-tching	43 "

TAI-WI-HOUAN.

Includes stars in Leo, Virgo, and Coma, and consists of two chains of five and six stars respectively in the present planisphere, but of five and five stars respectively in Grimaldi, Schlegel, and Williams.

No.	Chinese Name.	Star.
140.	Tai-yin-toung-mun	36 Comæ.
141.	Tchoung-hoa toung-mun	ϵ Virginis.
142.	Tai-yang toung-mun	δ "
143.	Tso-yi-mun	γ "
144.	Tso-tchi-fa	η "

No.	Chinese Name.	Star.
145.	Yeou-tchi-fa	β Virginis.
146.	Chang-tsiang	σ Leonis.
147.	Tsse-tsiang	ι „
148.	Tsse-siang	θ „
149.	Chang-siang	δ „
150.	Hou-fun	γ^2 „

TIEN-CHI-HOUAN.

Includes stars in Hercules, Ophiuchus, and Serpens, and consists of two chains of eleven stars each.

No.	Chinese Name.	Star.
151.	Wei	δ Herculis.
152.	Tchao	λ „
153.	Kiou-ho	μ „
154.	Yen	$\circ$ „
155.	Thsi	ζ Aquilæ.
156.	Ou	θ Serpentis.
157.	Siu	η „
158.	Toung-hai	ζ „
159.	Youe	ν Ophiuchi.
160.	Nan-hai	ξ Serpentis.
161.	Soung	η Ophiuchi.
162.	Tching	ζ Ophiuchi.
163.	Tsou	ϵ „
164.	Liang	δ „
165.	Pa	ϵ Serpentis.
166.	Chou	α „
167.	Thsin	δ „
168.	Tcheou	β „
169.	Han	γ „
170.	Tsin	κ Herculis.
171.	Ho-kien	γ „
172.	Ho-tchoung	β „

Note on the Regnal Years in the Elephantine Papyri.

By J. K. Fotheringham, M.A.

(Communicated by E. B. Knobel.)

Mr. Knobel has attempted, in *Monthly Notices*, lxix. pp. 8-11, to discover historically the dates of accession of the Persian kings, and by a comparison of these dates with the regnal years recorded in the Elephantine papyri, to discover the system by which the regnal years were reckoned. He mentions three different systems on which it is supposed that regnal years were reckoned at the period in question (the fifth century B.C.),—(1) from the accession of the king; (2) from the 1st. Nisan following the accession; (3) from the 1st. Thoth preceding the accession. I doubt, however, whether it would be possible to produce tangible evidence of any system in use at that date which did not reckon from the New Year's day following the accession, though the New Year's day may have been different in different countries and in different calendars.

The historical data which Mr. Knobel uses are unfortunately very faulty. He quotes Oppert for evidence that Darius was living in September 485 B.C. But Oppert's dates for the reign of Darius have been shown to be one year too low, and this date should be corrected to September 486 B.C.* He next asserts that Xerxes was assassinated by Artabanus in the beginning of the archonship of Lysitheus, in the 4th. year of the 78th. Olympiad, from which he infers that the assassination of Xerxes was not earlier than July 465 B.C. The date is apparently derived from Diodorus,† who gives the name of the Athenian archon and the Roman consuls, but does not specify the time of year. Diodorus' reputation as a chronologist for the period between the Persian and Peloponnesian wars is unfortunately very low, and in any case we do not know that the authority from whom his date is derived reckoned the year from the entrance of the archon on office. Diodorus, in fact, always identifies the Athenian official year which began in summer with the Roman official year, which appears to have begun at very different seasons at different dates. I do not think any reliance can be placed on this date.

Mr. Knobel is even more unfortunate when he attempts to date the accession of Artaxerxes from Thucydides. According to him, "Thucydides records that in the 4th. year of the 78th. Olympiad, July 465 B.C. to June 464 B.C., Themistocles went up the country,"

* See Professor Weissbach's article, "Über einige neuere Arbeiten zur babylonisch-persischen Chronologie," *Zeitschrift der deutschen morgenländischen Gesellschaft*, Band lv. (1901), pp. 195-220, especially p. 220; also his article, "Zur neubabylonischen und achämenidischen Chronologie," *ibid.*, Band lxii. (1908), pp. 629-647.

† xi. 69.

etc. As it happens, Thucydides* does not assign a date to this event and knows nothing of the reckoning by Olympiads.

Mr. Knobel's next citation is happier. He asserts that "the death of Artaxerxes is recorded by Thucydides as occurring in the winter of the archonship of Stratocles—the 4th. year of the 88th. Olympiad, about December 425 B.C." Here, again, Thucydides† says nothing about archons and Olympiads, but places the event in question in the winter of the 7th. year of the Peloponnesian war, *i.e.* in the winter of 425–4 B.C. This is consistent either with my view that Darius Nothus, who followed after the short reigns of Xerxes II. and Sogdianus, began to reign between Nisan (March or April) and Thoth (December) 424 B.C., or with Mr. Knobel's view that he began to reign in December 424 B.C. Similarly, the treaty between Sparta and Tissaphanes, which is one of the last events mentioned by Thucydides‡ in his full narrative of the winter 412–411 B.C., and which is dated in the 13th. year of Darius, might well fall within that regnal year, whether we reckon it with Mr. Knobel from December 412 B.C., from a spring New Year in 412 B.C., or from some other date which may have served as New Year's day in Caria or Lydia. Diodorus' date for the death of Artaxerxes and the accession and death of Darius Nothus would suit Mr. Knobel's dates and mine equally well.

Mr. Knobel briefly dismisses the theory that the regnal years are reckoned from 1st. Nisan after the accession by pointing out that in this case the date of Papyrus A, 12th. September 471 B.C., would not fall in the 15th. year of Xerxes, but this conclusion is based upon Oppert's date for the accession of Xerxes, which is, as has been seen, one year too low. There can be no doubt about the identification of the regnal years of Xerxes, because this period is covered by the eighteen years' list which extends into a period when astronomical dates are numerous. The 15th. year, according to the Babylonian reckoning, must have begun in Nisan 471 B.C., although the Babylonian regnal years are reckoned from 1st. Nisan. The earliest dated tablet in the reign of Xerxes belongs to 22nd. Arab-samma in the year of his accession, probably 1st. December 486 B.C. Ptolemy reckons his first year from 1st. Thoth = 23rd. December 486 B.C.

Mr. Knobel goes on to suggest that the years are reckoned from the 1st. Thoth preceding the accession, except where two different regnal years are given in the same papyrus, and here he admits that the lower regnal year is computed from Nisan. To this I should reply, that the papyri afford no evidence which would enable us to determine whether the dates reckoned from Thoth are computed from the Thoth preceding or the Thoth following the accession, though the latter theory is more consistent with the practice of the age. It is also easier to believe that the Jewish dates are reckoned from Nisan, and the Egyptian from Thoth. There are only three instances in the series where the two systems of reckoning would give different regnal years. In two of these

* i. 137.

† iv. 50.

‡ viii. 58.

three (J and K) both regnal years are given. In the remaining instance (B) two regnal years are given, but are not annexed to the different calendar dates, and presumably belong to one system of reckoning. Here we read "The 21st. year (of Xerxes), the beginning of the reign when Artaxerxes the king ascended the throne." Now there is no dispute that the 1st. year of Artaxerxes was the year following the 21st. year of Xerxes; if, therefore, Mr. Knobel were right in supposing that the 1st. year of Artaxerxes was reckoned from the New Year's day preceding his accession, it would follow that his accession would fall in the year after the 21st. of Xerxes, *i.e.* the 22nd. of Xerxes, continuing the enumeration of his years after his death. It is clear, therefore, that this date is not reckoned from a New Year's day preceding the actual accession. If Professor Schürer's identification, which I have accepted, be correct, the date (2nd. January 464 B.C.) falls into the 21st. year of Xerxes reckoned from Nisan, which is also the accession year of Artaxerxes. From this it would follow that the Jewish dates are computed from the New Year's day (in this case 1st. of Nisan) before the king's accession. Mr. Knobel's objection to Professor Schürer's date is, as has been seen, based upon a precarious interpretation of a historian chronologically untrustworthy. The date which he himself suggests (23rd. December 464 B.C.) is inconsistent with his own chronology, according to which the 2nd. year of Artaxerxes, corresponding to the 23rd. of Xerxes, should have begun on the 17th. of December 464 B.C. The date on this papyrus is interesting as being the earliest known date in the reign of Artaxerxes.

Mr. Knobel even goes the length of suggesting that the regnal years in Palestine in the time of Nehemiah were computed from the 1st. of Thoth, on the ground that Chisleu in the 20th. year of Artaxerxes preceded Nisan in the same year. Surely it would be easier to suppose that these years are reckoned, according to the Syrian and modern Jewish practice, from the autumn New Year's day, the 1st. of Tishri.

On the whole, I see no reason for abandoning the opinion that the Jewish dates on the Elephantine papyri are certainly, and the Egyptian probably, reckoned from the New Year's day preceding the actual accession of each king: in the case of the Jewish dates this New Year's day would be the 1st. of Nisan, and in the case of the Egyptian dates the 1st. of Thoth.

12 *Holywell, Oxford:*
1909 *March 6.*

On the Relation between Period and Density of Algol-Variables.

By the Rev. J. Stein, S.J., Sc.D.

(Communicated by Prof. H. H. Turner, D.Sc., F.R.S.)

1. As is well known, a maximum value of the mean density of an Algol-system can be derived from the period (P) and the total duration of eclipse ($2t_0$).* If the orbit is supposed to be circular, this maximum-value D is given by

$$D = \frac{K}{P^2 \sin^3 nt_0}; \quad n = \frac{2\pi}{P}, \quad (1)$$

where P and t_0 may be expressed in hours, D in the mean density of the Sun as unity.

This value is identical with the *real* mean density (δ) if the two stars are of the same size, and if the inclination of the line of sight to the orbit is zero.

In order to determine the constant K we put—

P = one year = 365.25×24^h ; $nt_0 = 32' \ 3''.64$ = mean apparent diameter of the Sun; consequently $D = \frac{1}{2}$ and $K = 31.17$.

Thus

$$D = \frac{31.17}{P^2 \sin^3 nt_0} \quad (2)$$

and 31.17 being $= 1.005 \pi^3$, we can bring this into the simple form—

$$D = \frac{P}{(2t_0)^3} \left(\frac{nt_0}{\sin nt_0} \right)^3 \quad (3)$$

Méridieu has shown that D is not very different from δ , if one star is not considerably larger in size than the other.

2. In the *Mitteilungen der Hamburger Sternwarte*, No. 11, Dr. Graff has deduced from his own observations the elements of the orbits of 10 Algol-variables. A slight extension was given to these by Professor Ristenpart (*Ast. Nach.*, No. 4250), who derived from the elements the mean density of the systems by the formula

$$\delta = \frac{a^3 C}{P^2 (1 + \kappa^3)} \quad (4)$$

where a is the radius of the relative orbit, κ the radius of the dark satellite, both expressed in the radius of the bright star as unity. If P is given in hours, then $C = \frac{1}{4}K$.

Arranging the stars according to decreasing periods, Ristenpart finds a nearly progressive increase of density; and he adds that this might be expected with regard to formula (4): "Natürlich

* M. Méridieu, "Densité des étoiles variables du type d'Algol," *Comptes Rendus de l'Acad. d. Sciences*, vol. 122 (1896), p. 1254.

laufen die Körper um so rascher um einander um, je dichter sie sind."

This argument is, of course, understood, "*cæteris paribus*"; strictly we can only infer from the results obtained that, with increasing P , the factor $\frac{a^3}{1 + \kappa^3}$ in general increases less rapidly than P^2 .

3. In order to examine more closely the relation between density and period, I have put together the following list of 38 Algal-stars, for which I could find the duration $2t_0$ of the eclipse, determined exactly enough for the purpose. The fourth column contains the maximum-density D ; the next one gives the mean densities δ , derived by Ristenpart from Graff's elements. The slight differences $D - \delta$ are an experimental proof that D is a fairly approximate value of δ . In the sixth column are given the values of $\frac{1}{D} = R$ (= "rarity").

Star.	P. h	$2t_0$. h	D.	δ .	R.
RZ Draconis (Z_2)	13.2	2.7	0.848		1.2
ZZ Cygni (Z_6)	15.1	3.5	.358		2.8
U Ophiuchi	20.1	5.1	.211		4.8
RX Herculis (X_2)	21.4	5	.226		4.4
RU Monocerotis (U_2)	21.6	5	.228		4.4
R Canis Mai.	27.3	5	.259		3.9
RZ Cassiopeiæ (Z_2)	28.7	5.5	.203		4.9
Z Draconis	32.6	4.7	.347	.345	2.9
RX Draconis (X_2)	45.5	5	.389		2.6
RW Monocerotis (W_2)	45.8	6.5	.190		5.3
RV Persei (V_2)	47.4	6.5	.194		5.2
δ Libræ	55.8	12	.041		24.4
RS Sagittarii (S_2)	58.0	10.4	.061		16.4
U Cephei	59.8	10	.069		14.5
RY Aurigæ (Y_2)	65.4	8	.138		7.3
143.1907 Andromedæ	66.4	8	.140		7.1
RW Tauri (W_2)	66.4	7.9	.148	.136	6.8
RW Geminorum (W_2)	68.8	12	.046		23.9
β Persei	68.8	10	.077		13.0
Y Cygni	71.9	8	.150		6.7
Z Persei	73.3	11.1	.060	.059	16.7
Y Camelopardalis	79.3	12	.051		19.7
WW Cygni (Z_5)	79.6	11.8	.053	.052	18.9
U Sagittæ	81.1	13.1	.041	.041	24.4
UW Cygni (Z_4)	82.8	10.5	.079	.075	12.7

Star.	P. h	2t ₀ . h	D.	δ.	R.
U Coronæ	82.8	9.7	.097		10.4
λ Tauri	94.9	10	.100		10.0
Z Herculis	95.8	9.2	.129		7.8
R Aræ	106.2	10.3	.103		9.8
SW Cygni (V ₃)	109.5	11.8	.070	.064	14.3
RR Delphini (R ₂)	110.4	14	.043		23.3
W Delphini	115.3	17.2	.025	.025	40.0
S Velorum	142.4	15.2	.043		23.3
SY Cygni (X ₃)	144.1	19.0	.023	.023	43.5
RY Persei (Y ₂)	164.7	26	.010		100.0
VW Cygni (V ₅)	202.3	20	.027	.020	37.1
S Cancri	227.6	21.5	.025		40.0
RR Puppis (R ₂)	250.3	17	.033		30.4

4. A rough preliminary calculation showed that there can be no question of proportionality between P^2 and R . On the other hand, the product $P D$ oscillated between relatively narrow limits. This seemed to make an investigation of the correlation between P and R more promising.

By taking direct square and product deviations from mean values we find:—*

Including all the 38 stars—

Mean period $\bar{P} = 82.66$ hours	Mean "rarity" $\bar{R} = 16.95$
Variability in period $\sigma_P = 55.60$ hours	Variability in rarity $\sigma_R = 16.28$
Coefficient of variability $V_P = .672$	Coefficient of variability $V_R = .960$

Correlation :

$$r_{R,P} = .733 \pm .051 \text{ (p.e.)}.$$

Probable value of rarity :

$$R = .215 P - .83 \pm 7.46 \quad . \quad . \quad . \quad (5)$$

Excluding 4 stars with period greater than 6 days, we have for 34 stars—

Mean period $\bar{P} = 61.65$ hours	Mean "rarity" $\bar{R} = 12.86$
Variability in period $\sigma_P = 34.11$ hours	Variability in rarity $\sigma_R = 10.13$
Coefficient of variability $V_P = .554$	Coefficient of variability $V_R = .788$

Correlation :

$$r_{R,P} = .731 \pm .054$$

Probable value of rarity :

$$R = .217 P - .51 \pm 4.66.$$

* For the notations used, see *M.N.*, Dec. 1908, vol. lxix. pp. 128 ff.

5. Thus the magnitude of correlation seems to indicate a real relationship between period and density, or, more exactly, *between period and duration of eclipse* (see Note at end). For a longer period (say > 3 days) we can then put with sufficient accuracy:

$$PD = \frac{1}{0.216} = 4.63,$$

or, according to (2):

$$P \sin^3 nt_0 = 6.73.$$

When P is very large, nt_0 must be small, hence $nt_0 = \sin nt_0$, and

$$(2t_0)^3 = .216 P^2. \quad (6)$$

Applying this, for instance, to the Algol-star 142, 1907 Cassiopeiæ, with a period of $36^d.56 = 877.4$ hours, we find:

$$2t_0 = 55 \text{ hours; } D = 0.005.$$

And, in fact, according to Enebo (*Ast. Nach.*, No. 4241), the duration of eclipse lies between 49.7 and 64.8 hours ($0.003 < D < 0.007$).

It is hoped that this result, in connection with the relations found by Professor Karl Pearson and Miss Julia Bell, may throw some new light on the nature and evolution of this most interesting class of variable stars.

Specola Vaticana:
1909 January 28.

Note.—As was remarked in No. 5, the relation found between P and D is, properly speaking, a relation between the period and the observed duration of eclipse. This point was especially emphasized by Professor Karl Pearson, to whom the MS. was submitted by Professor Turner. He makes the following interesting remark:—

“With reference to the formula

$$D = \frac{31.17}{P^2 \sin^3 (nt_0)}; \quad n = \frac{2\pi}{P},$$

if t_0 be a given function of P , it follows that D would be absolutely determined by P . The fundamental question, therefore, is what is the relation between period and eclipse time? I think these must be very closely related. I have accordingly worked them out, and find

$$r_{P,t_0} = +.877.$$

“It will thus, I think, be seen that D being a mathematical function of P and t_0 , not the true density, the correlation of D and P is a secondary result of the fundamental relation of the observed quantities P and t_0 . In fact, the value of $r_{P,P}$ can be deduced at once from the values of $\bar{P}$, $\bar{t}_0$, σ_P , σ_{t_0} and r_{P,t_0} .”

According to Professor Pearson's remark, putting together the elements of correlation between P and t_0 , we get—

Mean period	$\bar{P} = 82.66$	Mean Eclipse time	$2\bar{t}_0 = 10.532$
Variability in period	$\sigma_P = 55.60$	Variab. in m.E.t.	$\sigma_{2t_0} = 5.236$
Coefficient of variab.	$V_P = .672$	Coeff. of variab.	$V_{2t_0} = .497$

Correlation :

$$r_{P,2t_0} = .877 \pm .025 \text{ (p.e.)}.$$

Probable value of eclipse time,

$$2t_0 = .0826 P + 3.70 \pm 1.70 \text{ (p.e.)} \quad (7)$$

This value of $2t_0$, between the limits considered, will, of course, not differ substantially from that given by the formula (5) :

$$R = \frac{P^2 \sin^3 nt_0}{31.17} = 0.215P - 0.83. \quad (5)$$

This is easily shown by means of the approximate formula (6') :

$$2t_0 = 0.594 P^{2/3}. \quad (6')$$

In fact, if we wish to replace the curve

$$y = cx^{2/3}; \alpha < x < \beta$$

approximately by a straight line

$$y = a + bx; \alpha < x < \beta,$$

the best fitting values of a and b , according to the method of least squares, are given by the equations

$$\begin{aligned} a \int dx + b \int x dx &= c \int x^{2/3} dx \\ a \int x dx + b \int x^2 dx &= c \int x^{5/3} dx \end{aligned}$$

where the integrals are to be taken between the limits α and β .

Putting $y = 2t_0$, $x = P$, $c = 0.594$, $\alpha = 25$, $\beta = 250$, we find

$$2t_0 = 0.0797 P + 4.50$$

in close agreement with (7).

The following table gives in the 2nd. and 3rd. columns the values of $2t_0$, derived from formulæ (5) and (7) :

P	$(2t_0)_5$	$(2t_0)_7$	Diff.
h	h	h	h
25	5.2	5.8	— .6
50	8.5	7.8	+ .7
75	11.0	9.9	+ 1.1
100	13.4	12.0	+ 1.4
150	17.4	16.1	+ 1.3
200	20.8	20.2	+ .6
250	24.4	24.3	+ .1

Radial Movement in Sun-spots. By J. Evershed.

Displacements of the absorption-lines of hydrogen and calcium are frequently observed in all spot disturbances where there is active change in progress, and during the genesis of a spot. These line-shifts may amount to several Ångström units in either direction in the spectrum, indicating velocities towards or away from the observer of one or more hundred kilometres per second. Only the elements of the higher chromosphere seem to be affected as a rule, and the movements are seldom maintained for more than a few minutes at a time; also they are rarely found within the penumbral area of a spot.

Recently line-shifts of quite another character have been photographed here. These are permanent, affect most, if not all, of the Fraunhofer lines, and are greatest in the penumbrae.

In some spectra obtained by me this year, in the fourth and fifth orders of a Rowland plane grating of 3·2 inches ruled surface and 14,428 lines to the inch, the lines crossing the spot-band were found to be slightly bent, or inclined one or two degrees as compared with the lines in the neighbouring photosphere. The first plate showing this feature clearly was exposed on January 7, 1909, in the fourth order of the grating. It covers the region $\lambda 4650$ to $\lambda 4790$, and the scale is $1\text{ Å} = 1\cdot08$ mm. The total displacement of the lines measured on each side of the penumbra amounts to $0\cdot027\text{ Å}$, indicating a receding velocity on the north-west side of the spot of $0\cdot85$ km. per second, and an approaching velocity of the same amount on the south-east side. The spot was in latitude 9° north and longitude 31° west of the central meridian, and the position-angle of the north end of the spectrograph slit was 314° .

The appearance of the lines on this plate at once suggested a rotation of the absorbing gases in the penumbra—not a vortex motion, but a rotation of the spot as a whole about a point at its centre. The lines are quite straight over the spot, although inclined to the undisturbed lines; and in the centre of the umbra there is no displacement. The hypothesis of circular motion of any kind has proved, however, to be certainly untenable. From an examination of about 150 spectra, obtained since January 7, and representing seven different spots in the northern hemisphere and four in the southern, the following definite results have been obtained:—

1. All the spots examined show line-shifts of about the same order of magnitude when at the same distance from the centre of the Sun's disc.

2. The displacements disappear when the spot is within 10° of the centre.

3. The displacements are most marked when the spot is between 30° and 50° east or west of the meridian.

4. The displacements are of opposite sign on opposite sides of the central meridian.

5. The displacements are invariably towards the violet on the preceding side of a spot and towards the red on the following side, when the spot is east of the central meridian; the reverse when west.

6. Southern spots show the same directions of movement as northern.

7. No displacements are obtained when the slit bisects a spot in a direction at right angles to a line joining the spot and the centre of the Sun's disc.

A hypothesis which seems in harmony with all the facts here stated is one which attributes the displacements to a radial movement outwards from the spot centre. The motion must be essentially horizontal, or parallel to the Sun's surface; this is shown by the total disappearance of the line-shifts when the disturbance is near the centre of the disc. The hypothesis of a vortex, or rotation of any kind, about an axis perpendicular to the Sun's surface, is negatived by the fact stated in paragraph 7, for it is evident that for a circular movement a nodal point should be found when the slit bisects the spot in a direction passing through the centre of the Sun's disc, the maximum displacement occurring in a direction at right angles to this. This position of the node, however, differs from that actually found by about 90° .

To obtain evidence on this crucial point, advantage is taken of the rotation of the solar image, due to the action of the heliostat. Thus, in the interval between 8 A.M. and 5 P.M. the position-angle of the slit, which is fixed truly vertical, changes through about 100° , the north end of the slit passing from position-angle 300° through 0° to 40° , being at 0° at solar noon. Different sections of a spot can therefore be obtained by simply taking a succession of photographs at intervals throughout the day. Now, it has invariably been found that the displacements diminish in amount from 8 A.M. up to about 11, when they usually disappear entirely, reappearing with opposite signs in the afternoon; but the time of no displacement does not occur exactly at solar noon, and the evidence so far obtained shows that the line of nodes coincides with the direction at right angles to a line joining the spot and the centre of the disc, as it should do if the motion is radial. The whole of the evidence obtained up to the time of writing will be published in Bulletin No. 15 of this observatory, and need not be detailed here: but, in view of the importance of the determination of the line of nodes, I give a table showing the results of seven independent determinations.

Date.	Kodaikanal Spot No.	P.A. of Spot.	P.A. of Node.	Angle between Node and Centre of Disc.
Jan. 18	1588	82° E.	20° W.	102°
„	1591	74° E.	2° W.	76°
20	1591	60° E.	23° W.	83°
25	1594	87° E.	8° W.	95°
„	1591	84° W.	0°	84°
26	1591	88° W.	9° E.	97°
27	1595	60° E.	31° W.	91°
				Mean 90°

The estimate of the time when there is no shift must necessarily be extremely uncertain, considering the small amount of the displacements, even when at their greatest; and the accordance in the above table is really surprising, and testifies to the sensitiveness of the eye in estimating very minute deviations from straightness in the spectrum lines.

Since these determinations were made, a device has been added to the spectrograph, by which the Sun's image can be made to rotate on the slit plate through 90°, and the results obtained by this means leave no doubt whatever as to the position of the line of nodes; for wherever a spot may happen to be situated on the disc, outside the limit of 10° from the centre, at which the shift becomes inappreciable, the displacement is always found to be greatest when the slit bisects a spot in the direction of the centre of the disc. If the image is turned through 90°, so that the bisection is at right angles to the direction of the centre, no shift is obtained, the lines remaining absolutely straight across the spot-band.

The constancy of the displacement in all the spots examined is most remarkable; and the fact that it has not, to my knowledge, been observed previously is difficult to explain, except by supposing that spot spectra are usually obtained when near the central meridian, or with the slit approximately in the line of nodes.

It may be well, perhaps, to mention that a test of the reality of these minute line-shifts was made by photographing the spot spectrum in the red, in the region including the well-known group of solar and telluric lines used by Dunér and Halm in their determinations of the solar rotation. In this group, the telluric oxygen lines are very narrow and well defined, whilst the solar iron lines are comparatively broad, and are indeed much widened in spots. Since the displacements are much more conspicuous in the finer lines of the spectrum, the oxygen lines should show the effect strongly if spurious, but in the photographs they are found to be absolutely straight, whilst the iron lines beside them are inclined.

The maximum total displacement obtained with the slit in the direction of the centre of the disc for a spot 40° west of the central

meridian and 9° north is 0.040 Ångstrom units at $\lambda 4850$. The component of velocity in the line of sight from this is 1.24 km. per second towards and away from the observer, on the following and preceding sides of the penumbra respectively. Dividing this by the sine of the angular distance of the spot from the centre of the disc ($\sin 42^{\circ} = .67$) gives an actual motion parallel to the Sun's surface of 1.9 km. per second each way. But the motion seems to vary directly as the distance from the spot centre, and the value found is for the outer limits of the penumbra, or at an actual distance of about $32,000$ km. from the centre of the spot. At intermediate distances, the motion will be less in proportion.

It is somewhat disappointing, perhaps, that the hypothesis of a radial movement, which is so strongly supported by these observations, seems entirely out of harmony with the splendid discovery of the Zeeman effect in sun-spots, made by Professor Hale. This seems to demand a vortex, or at any rate a circular movement, in sun-spots; and it was only after a considerable amount of evidence had accumulated that the preconceived conviction that the motion must be circular was abandoned.

A consoling feature of this new theory of spot movement is that it seems to explain the radial structure of the filaments in the penumbrae of spots, and the radial disposition of the calcium flocculi immediately surrounding a well-developed symmetrical spot. It also harmonises with the well-known tendency of the principal spots in a group to separate, the leader advancing and the follower receding.

A difficulty should also be mentioned. When the slit centrally bisects a symmetrical spot in a direction approximating to that giving the greatest shift, the displaced lines appear quite straight, as before mentioned, and inclined to the undisturbed lines, the greatest shift occurring at the outer limits of the penumbra. This seems to imply an accelerating movement from the centre of the spot outwards; yet at the limits of the penumbra the motion apparently ceases abruptly. It is hoped that further research will throw light on this and other obscure points.

Kodaikānal :
1909 February 6.

The Brightness of Saturn with Ring Invisible.

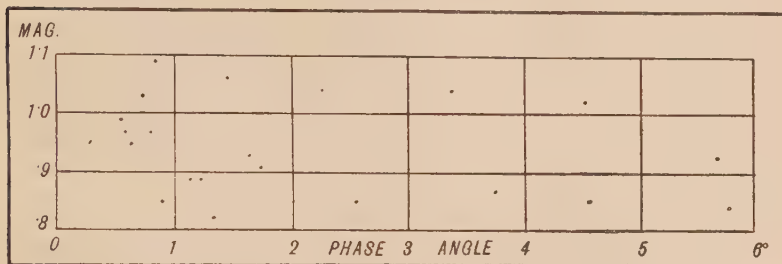
By J. M. Baldwin, M.A.

(Communicated by Sir David Gill, K.C.B.)

In a paper on photometric measurements of Saturn which appeared in *Monthly Notices*, vol. lxviii. p. 368, owing to an unfortunate mistake, a factor 2 had been omitted in obtaining the correction to be applied to the observed brightness to reduce it to the brightness in mean opposition, and in consequence the correction given is only half as great as it should be. In the following table the correct values are given, the observations being arranged in order of phase angle, the columns being respectively date of observation, phase angle, observed difference of magnitude Saturn - α Aquilæ, correction to mean opposition, and the magnitude reduced to mean opposition, the magnitude of α Aquilæ being taken as 0.96 (Müller, *Potsdam Publ.*, viii. p. 235, 1893). Further particulars of the observations are given in the paper above cited, the first four observations in the former table having been omitted, for reasons there mentioned.

Date.	α .	ΔM .	Corr.	M_0 .	Corr.	M.
1907	.					
Sept. 18	0.28	- .01	- .02	.93	+ .02	.95
13	0.53	+ .02	- .02	.96	+ .03	.99
22	0.59	+ .02	- .02	.96	+ .01	.97
12	0.63	- .02	- .02	.92	+ .03	.95
11	0.73	+ .06	- .02	1.00	+ .03	1.03
Sept. 24	0.80	+ .03	- .02	.97	.00	.97
10	0.83	+ .13	- .02	1.07	+ .02	1.09
25	0.90	- .09	- .02	.85	.00	.85
27	1.11	- .04	- .02	.90	- .01	.89
28	1.21	- .04	- .02	.90	- .01	.89
Sept. 29	1.32	- .10	- .02	.84	- .02	.82
4	1.46	+ .12	- .03	1.05	+ .01	1.06
Oct. 2	1.63	+ .02	- .03	.95	- .02	.93
3	1.74	.00	- .03	.93	- .02	.91
8	2.25	+ .13	- .03	1.06	- .02	1.04
Oct. 11	2.55	- .04	- .04	.88	- .03	.85
20	3.39	+ .16	- .05	1.07	- .03	1.04
24	3.74	+ .01	- .06	.91	- .04	.87
Nov. 3	4.51	+ .19	- .09	1.06	- .04	1.02
27	5.69	+ .20	- .17	.99	- .06	.93
Dec. 1	5.78	+ .13	- .19	.90	- .06	.84

To find if the observations show any variation of the brightness of the spheroid of Saturn with the phase angle, the observations have been further reduced by applying the reduction to "ring invisible" given by Seeliger in *Ast. Nach.** The reductions and the reduced values are given in the last two columns of the table. These reduced values are plotted against phase angle in the



accompanying diagram, and from this it appears that the alteration in brightness of the spheroid with phase angle, if any, must be very slight, and many more observations would be necessary to determine its magnitude.

Observatory, Melbourne :
1909 January 20.

Note on the remarkable Meteor of 1909 February 22, 7^h 30^m.
By W. F. Denning.

A meteor of very exceptional character presented itself in the sky on Monday, February 22 last, passing in a nearly E. to W. direction above the English Channel, with a long flight, performed at moderate velocity and height. It was observed as far north as Nottingham, as far east as Colchester; and reports have come to hand from Plymouth, from places in South Wales, from the Channel Islands, and from Cherbourg, Brest, and other stations in the north of France.

The meteor was a fine one, of a blue colour, and it gave a succession of bright outbursts during the latter portion of its flight. At the end, its westerly direction appears to have been suddenly checked, and it turned earthwards, its incandescent material apparently spreading out and falling several degrees in a zigzag or wavy course.

The reddish train of sparks which flowed from the nucleus of the meteor quickly died away, but there immediately came out along the course a vivid streak of phosphorescence, which

* "Über die Helligkeit des Saturn bei verschwundenem Ring," H. Seeliger, *A.N.*, 4263, clxxviii. 250, 1908.

intensified and lengthened, and at once began to drift in a direction to the N.W.* Many observers estimated the length of the streak as between 100° and 120° between 8 and 8.30, but this does not include the curious bends at its extremities. At several places the silvery beam or ribbon of light evolved from the meteor was watched for 2 hours: at Plymouth it was distinctly visible for $2\frac{1}{2}$ hours, and another observer 4 miles off Start Point, on the S. Devon coast, followed it for 3 hours. As seen from the south of England, it passed from the stars of Canis Minor and Major, Monoceros and Orion, to Ursa Minor, Cepheus, Cassiopeia, and the constellations lying S.W. It widened from about $\frac{1}{4}^{\circ}$ to 2° or 3° at last.

The streak was so bright and so extensive that it attracted the earnest attention of thousands of persons who did not see the flight of the meteor at all. This flight was directed from a point in Coma Berenices ($190^{\circ} + 20^{\circ}$) lying low down over the E.N.E. horizon at the time. There is a pretty well-known radiant here in the spring months, and it is composed of slow, trained meteors. A large number of observations have been received both of the fireball of February 22 and its long-enduring streak, and it is hoped to give more definite particulars concerning the main features of the phenomenon in a subsequent paper.

Bishopston, Bristol:
1909 March 11.

Comparison of Ancient Eclipses of the Sun with Modern Elements of the Moon's Motion. By Simon Newcomb.

The passages in the writings of ancient authors supposed to refer to total eclipses of the Sun have been so fully discussed during the last few years, especially by Cowell and Nevill in the *Monthly Notices*, and quite recently by Mr. Fotheringham, that the subject is fairly well thrashed out so far as the question of interpretation is concerned. Most of the supposed eclipses on which stress was laid by Airy, Hansen, and other older authorities, in testing the lunar tables, have been nearly eliminated from consideration by doubts and inconsistencies of various kinds. The only one of these I need mention is the eclipse of Thales. The questions associated with this eclipse may now be considered as well cleared up. From the corrections which I have applied to the lunar elements, it would appear that the Sun set upon the combatants only a short time, perhaps fifteen minutes, before the total phase commenced. Thus the accuracy of the phraseology used by Herodotus, and in-

* For an hour or more after its first projection the beam or streak was brighter than any part of the Milky Way, and its shape varied under the action of wind currents at different altitudes. One observer at Lymington describes the streak as like "a brushful of starlight from the Milky Way drawn across the sky."

deed the whole story as he narrates it, seemed to be confirmed in a remarkable way. But the eclipse still remains useless for any astronomical purpose.

After eliminating all the material from which no result can at present be reached, there remain only seven eclipses, the five discussed by Mr. Cowell in the *Monthly Notices*, vol. lxxv. pp. 861-867, and lxxvi. p. 3, the eclipse of Agathocles, and that of -128, November 20, which Mr. Fotheringham has shown is probably that described as total in the Hellespont while the Sun was four-fifths eclipsed at Alexandria. It was pointed out by Celoria that this last eclipse might have been that of Agathocles, but the year -128 is more probable.

The course by which a conclusion can best be reached is to begin with a careful computation from the best modern theory, and take as the basis of discussion a comparison of the results thus reached with the statements of the historians. This method has not been that hitherto adopted. Oppolzer and Ginzel, in their respective *Canonen*, and Mr. Cowell in his work, start with expressions for the lunar elements constructed so as to represent the eclipses, without reference to their compatibility with modern theory and observations. I need only remark on this that I feel it difficult to be sure of any conclusion reached in this way, and the difficulty is not lessened by the fact that the elements used by the three authorities I have named have no feature in common except that all diverge from what we suppose to be the most probable result of modern theory and observation.

The elements of the Moon's mean longitude which I have used are those derived in the general discussion described in my paper in the *Monthly Notices*, vol. lxxix. p. 164, of which the modern basis consists of observed occultations up to 1908. Hansen's motion of the perigee for 1800 has been adopted without change, but its secular acceleration is determined from Brown's theory. The same is true of the node, except that its motion has been corrected by +11" per century, as in Oppolzer's eclipse tables.

The corrections to the mean longitude of Hansen's *Tables de la Lune*, as finally adopted, have the following numerical values. The constant corrections for 1800 have not been applied except in the case of the node, because they are without importance for the present purpose.

Mean Long.	- 26".57	T - 4".231	T ² - 0".0058	T ³
Long. Perigee		- 0".966	T ² - 0".0078	T ³
Long. Node	+ 11".00	T - 0".629	T ² - 0".0013	T ³

T, the time, is reckoned from 1800.0 in Julian centuries. The Sun's mean longitude which I use is that of my own tables.

In computing the eclipses I have used Oppolzer's eclipse tables,*

* Syzygien-Tafeln für den Mond . . . Publication der Astronomischen Gesellschaft, No. xvi., Leipzig, 1881.

applying the corrections necessary to reduce the elements on which they are based to the adopted values. These corrections are as follows:—

$$\begin{aligned} 10^4 \Delta T &= 5^d.877 T + 1^d.0460 T^2 + 0^d.0011 T^3 \\ 10^4 \Delta L' &= - 2^\circ.17 \quad ,, - 0^\circ.050 \quad ,, \\ 10^4 \Delta g &= - 73^\circ.80 \quad ,, - 13^\circ.79 \quad ,, \\ 10^4 \Delta \omega &= 0^\circ.00 \quad ,, + 1^\circ.50 \quad ,, - 0^\circ.021 \quad ,, \\ 10^4 \Delta g' &= - 59^\circ.3 \quad ,, + 0^\circ.61 \quad ,, \end{aligned}$$

These corrections of L' , g , g' and ω are for any fixed time. To reduce them to the values for mean conjunction, which form the arguments of the tables, we must apply their motions during ΔT .

The actual corrections to Oppolzer's arguments then become—

$$\begin{aligned} 10^4 \Delta P = 10^4 \Delta Q &= + 3^\circ.95 T + 1^\circ.55 T^2 - 0^\circ.004 T^3 \\ 10^4 \Delta I &= + 3^\circ.31 \quad ,, - 0^\circ.13 \quad ,, + 0^\circ.019 \quad ,, \\ 10^4 \Delta II &= - 50^\circ.6 \quad ,, + 0^\circ.47 \quad ,, + 0^\circ.001 \quad ,, \\ 10^4 \Delta III &= + 8^\circ.7 \quad ,, + 3^\circ.5 \quad ,, \\ 10^4 \Delta IV &= + 53^\circ.9 \quad ,, - 0^\circ.6 \quad ,, \\ 10^4 \Delta V &= - 47^\circ.3 \quad ,, + 0^\circ.3 \quad ,, \\ 10^4 \Delta VII &= + 57^\circ.2 \quad ,, - 0^\circ.7 \quad ,, \\ 10^4 \Delta VIII &= - 44^\circ.0 \quad ,, + 0^\circ.2 \quad ,, \end{aligned}$$

These corrections to Args. I to VIII are expressed in centesimal degrees, as in Oppolzer's tables. That of Arg. VI is unimportant. All are applicable to the body of Oppolzer's tables, and replace his empirical corrections given in the first four pages of his tables.

The method of computing the eclipses has been to take as fundamental elements the vertical ecliptical ordinate of the shadow-axis at the moment of true conjunction in longitude, together with the hourly motion of each coordinate. The results are then transformed at once to equatorial coordinates x and y , expressed in the Besselian form. But the zero of time is first transferred from T to some moment near the time of greatest phase. The coordinates of the point of observation, ξ and η , and their hourly motions, ξ' and η' , being also computed, the nearest approach of the shadow-axis to the point of observation is readily found. It will be seen that I have ignored the geographical position of the shadow-path, which is not required for our purpose.

The next step is to express the increment of minimum distance in terms of corrections to the elements. The formulæ of computation are developed in my *Researches on the Motion of the Moon*. For convenience of verification I cite them.

T being the G.M.T. of true conjunction in longitude, we choose a moment $T + t$, expressed in hours, near that of greatest phase at

the place. For this time we find the local West hour-angle of the shadow-axis

$$\mu' = (T + t) \times 15^\circ - Z + \lambda$$

Z being the equation of time, and λ the East longitude of the place.

The coordinates ξ and η and their hourly motions are then found from

$$\begin{aligned}\xi &= \rho \cos \phi' \sin \mu' \\ \eta &= \rho \sin \phi' \cos d - \rho \cos \phi' \sin d \cos \mu' \\ \xi' &= [9.4192] \rho \cos \phi' \cos \mu' \\ \eta' &= [9.4192] \xi \sin d\end{aligned}$$

where d is the declination of the shadow-axis, for which we may take that of the Sun.

For the Besselian (equatorial) coordinates of the shadow-axis, on the fundamental plane we need the Sun's Dec. and the angle h which its motion in longitude makes with the parallel of declination. For these we have, putting ϵ for the obliquity,

$$\begin{aligned}\cos d \sin h &= \sin \epsilon \cos L' \\ \cos d \cos h &= \cos \epsilon \\ \sin d &= \sin \epsilon \sin L'\end{aligned}$$

Here we may correct L' for the motion during t , but the effect of the correction will be small.

The ecliptical coordinates of the shadow-axis at the time T in terms of the Oppolzer elements are—

$$x_2^{(0)} = 0; \quad y_2^{(0)} = p \sin P$$

The hourly motions of these coordinates, which we regard as constant, are—

$$x'_2 = \Delta L; \quad y'_2 = q \cos Q$$

Their values at the time $T + t$ are

$$x_2 = x'_2 t; \quad y_2 = y_2^{(0)} + y'_2 t$$

The equatorial coordinates are

$$\begin{aligned}x &= x_2 \cos h - y_2 \sin h \\ y &= x_2 \sin h + y_2 \cos h\end{aligned}$$

which may be computed with the values of x_2 and y_2 for the moment $T + t$. A familiar method of computing x , y , and their hourly motions is

$$\begin{aligned}m \sin M &= x_2; & n \sin N &= x'_2 \\ m \cos M &= y_2; & n \cos N &= y'_2 \\ x &= m \sin (M - p); & x' &= n \sin (N - p) \\ y &= m \cos (M - p); & y' &= n \cos (N - p)\end{aligned}$$

We now find Δ , the minimum distance of the point of observation from the shadow-axis, by the equations

$$\begin{aligned} X &= x - \xi & X' &= x' - \xi' \\ Y &= y - \eta & Y' &= y' - \eta' \\ \Delta &= \sqrt{X'^2 + Y'^2} \end{aligned}$$

Finally, we have to know the effect of a change in the elements upon Δ . The only elements as to which there can be any question are the longitudes of the Sun, the Moon and the node. If we put

v, β, π , the moon's longitude, latitude, and parallax;
 L' the Sun's longitude;

we compute S from

$$\tan S = \frac{Y'}{X'} \dots (90^\circ > S > -90^\circ)$$

Then

$$\begin{aligned} \frac{\delta \Delta}{\delta v} &= - \frac{\delta \Delta}{\delta L'} = \frac{\sin(p - S)}{\sin \pi} \\ \frac{\delta \Delta}{\delta \beta} &= \frac{\cos(p - S)}{\sin \pi} \end{aligned}$$

We have to replace v and β by λ , the Moon's mean longitude, and Ω , that of the node. We have, with all necessary precision, when the Moon is near conjunction,

$$\frac{\delta v}{\delta \lambda} = 1.019 + 0.131 \cos g \equiv k$$

where

$$g = \text{Arg. } I \times 0^\circ.9$$

Then

$$\begin{aligned} \frac{\delta \Delta}{\delta \lambda} &= k \left(\frac{\delta \Delta}{\delta v} \pm 0.0895 \frac{\delta \Delta}{\delta \beta} \right) \\ \frac{\delta \Delta}{\delta \Omega} &= \mp 0.0895 \frac{\delta \Delta}{\delta \beta} \end{aligned}$$

The upper sign is used when the eclipse is near the ascending node, and the lower one near the descending node.

The results of the whole computation are tabulated as follows. T, L' , etc., are the elements from my theory in Oppolzer's notation. Δ is given in units of the 4th. place of decimals of the Earth's equatorial radius. Each Δ is followed by the increment which it would receive from an increment of λ , the Moon's mean longitude at the time, of Ω , the longitude of the node, and of L' the Sun's true longitude, all, for convenience, expressed in minutes of arc.

Elements of Seven Ancient Eclipses.

	T.	L.	Z.	P.	Q.	log p.	log q.	log ΔL.
- 1062	July	30°7548	116°332	+0°11	177°858	175°90	0°7005	8°7490 9°7528
- 762	June	4°8691	74°585	-2°08	176°547	174°74	0°6988	8°7509 9°7555
- 647	April	5°8765	8°925	+0°75	171°597	172°14	0°6909	8°7594 9°7638
- 430	Aug.	3°1327	124°487	+0°52	170°577	172°95	0°7233	8°7262 9°7277
- 309	Aug.	14°8505	136°573	+0°52	3°736	2°66	0°6929	8°7574 9°7612
- 128	Nov.	20°0565	236°080	-2°79	9°403	11°40	0°6998	8°7490 9°7509
+ 197	June	2°9958	71°068	-1°61	1°702	4°03	0°7245	8°7249 9°7268

Node.	Date.	Minimum Distance of Shadow-axis.				Radius of Shadow.
☾ - 1062	July 30	Δ = +516 - 16°2Δλ + 15°0Δ	ΔL'	- 0°3ΔL'		+ 94
☾ - 762	June 15	„ +684 - 3°3	+14°8	-11°9		+151
☾ - 647	April 6	„ +567 - 25°7	+14°6	+7°7		+178
☾ - 430	Aug. 3	„ +577 + 23°4	+15°3	-38°7		- 55
☾ - 309	Aug. 14	„ - 227 - 7°9	-14°6	+21°5		+145
☾ - 128	Nov. 20	„ +018 - 20°9	-14°7	+33°6		+ 15
☾ + 197	June 3	„ - 436 - 14°3	-15°6	+30°0		- 6

The preceding eclipses, that of - 128 excepted, were discussed by Mr. Cowell. He shows that five of the eclipses in question can all be represented in two ways, either by a change in the secular acceleration of the node, or by a hitherto unsuspected acceleration of the Sun's longitude, combined with an equal correction to the Moon's secular acceleration.

It will be seen that the preceding equations are confirmatory of Mr. Cowell's results. A diminution of 36' in the longitude of the node will make all five of the eclipses nearly or quite annular or total as the case may be. This effect will be brought about by a diminution of about 3".5 in the secular acceleration of the node.

In the equations it will be seen that the sums of the coefficients of the increments are nearly evanescent. It follows that the representation of the eclipses will remain nearly unchanged when we assign arbitrary equal increments to the longitudes of the Moon, the Sun, and the node. Hence, instead of a diminution of 3".5 in the nodal acceleration, we may assign an increment of 3".5 to the accelerations of the Sun and Moon. All this is quite accordant with Cowell's conclusions.

On the other hand, Cowell's corrections make both the eclipse of - 309 and that of - 128 only partial in the Hellespont. We have therefore to set against the evidence of the five the contrary evidence afforded by the eclipse of - 128 that the elements admit of no correction.

It may also be remarked that an increase of 1" in the secular acceleration of the mean longitude, coupled with a diminution of 1" in that of the node, would suffice to make the eclipses of - 1062 and - 647 total, without throwing that of - 128 quite away from

the Hellespont. But this change in $\Delta^2\Omega$ is outside the limit of theoretical uncertainty, and in $\Delta^2\lambda$ is difficult to admit.

The question whether the remarkable coincidence among the five eclipses affords probable evidence that either set of corrections to the elements is real is one on which opinions will doubtless differ. It must certainly be admitted that either correction seems extremely improbable. In the case of the node, gravitational theory admits of no appreciable correction to the adopted secular acceleration. That any other cause than gravitation changes the motion of the node is rendered improbable by the close agreement between the actual motion and the theoretical motion derived by Brown.

Equally difficult of explanation on any theory is a secular acceleration of the Earth's orbital motion. Moreover, while modern observations, so far as I have discussed them, do not exclude the possibility of such an acceleration, they do render it improbable.

The other branch of the question is, whether a chance coincidence in five cases out of six can be regarded as conclusive. The evidence in favour of the actual centrality at the several places of record of the eclipses, as the accounts are cited by Mr. Cowell, seems to me rather weak. It is certain that the eclipses were central within a few hundred miles of the several points, but, in the absence of any specific statement of the place, I see nothing stronger than a greater or less presumption of centrality at the supposed places.

The eclipse of the list in which the question of the phase is most perplexing is that of -430 , described by Thucydides, and also mentioned by Cicero and Valerius Maximus, as seen at Athens. Both of these accounts are weak from proving too much, namely, a total eclipse. If there could be any question whether the eclipse was annular or total, the combined weight of the two accounts would lead us to regard totality at Athens as practically certain, because no other phase could have been one of darkness, caused alarm among the citizens, or made the stars visible. We must therefore make a large allowance for exaggeration; and the question is whether this allowance can be much smaller for an annular eclipse than for the partial eclipse which is computed from the tables. The most exact basis for a comparison is afforded by comparing the fraction of the Sun's disk which must have remained uncovered in the two hypothetical cases. I find that if the eclipse was annular at Athens, the breadth of the annulus was about $18''$. If it were partial, a rough computation shows that the area of the Sun's disk uncovered was about four times that of the annulus—possibly nearly five times. At first sight this difference might seem important, but a little consideration may lead us to minimise this importance. The difference in the intensity of daylight under a cloudy sky may well be much greater than this without being striking. The light of the sky a very few minutes after sunset is less than it was during either of the hypothetical phases. Those who have observed total eclipses of the Sun know how little

striking is the apparent diminution in the intensity of daylight up to within a few minutes of the total phase, and how suddenly the first ray of the reappearing Sun illumines the sky and blots out the stars. Computation shows that Venus was very favourably situated for observation during the eclipse of - 430, and it may well be doubted whether any other star or planet could have been visible, even if the phase were annular. It is also to be considered that the ratio of the illuminations of the sky in the two cases is less than that of the uncovered Sun. Altogether, we may make an ample allowance for exaggeration and misapprehension without overstepping the bounds of reasonable probability.

Note on Professor Newcomb's Paper on "Comparison of the Ancient Eclipses of the Sun with Modern Elements of the Moon's Motion." By J. K. Fotheringham, M.A.

(Communicated by Professor H. H. Turner, D.Sc., F.R.S.)

[This note was written before the paragraph was added to Professor Newcomb's paper: "It may also be remarked that an increase of 1" in the secular acceleration of the mean longitude . . . is difficult to admit."]

Of the eclipses discussed in Professor Newcomb's paper, two only, those of - 1062 and - 128, are definitely described as total, within a closely defined locality. It has therefore occurred to me to seek values which will satisfy these two eclipses. I have to thank Professor Turner for kind assistance in the following discussion.

Professor Newcomb's equations for these eclipses are

$$- 1062 \quad - 16 \Delta \lambda + 15 \Delta \varpi - 0 \Delta L' + 516 = 0.$$

$$- 128 \quad - 21 \Delta \lambda - 15 \Delta \varpi + 34 \Delta L' + 018 = c$$

These give practically

$$\Delta(\lambda - \varpi) = \frac{516}{16} = 32.$$

$$\Delta(L' - \varpi) = \frac{21 \times 32 - 18}{34} = \frac{654}{34} = 19.$$

But this ignores the fact that these quantities increase as t^2 . If we adopt their values for the second eclipse as standard (roughly 2000 years ago), the first eclipse (which was 3000 years ago) must

have factor $\frac{9}{4}$ introduced into the variable terms. The equations then become

$$\Delta(\lambda - \Omega) = 32 \times \frac{4}{9} = \frac{128}{9} = 14.$$

$$\Delta(L' - \Omega) = \frac{21 \times 14 - 18}{34} = \frac{276}{34} = 8.$$

These are to be multiplied by the appropriate values of t^2 , referred to -128 as standard. Professor Newcomb's equations may then be written with these values:—

Date.	Factor for t^2 .		Δ	Radius of Shadow.
- 1062	2'2 ×	- 16 $\Delta(\lambda - \Omega)$ - 0 $\Delta(L' - \Omega)$	+ 516 = + 13	+ 94 Total.
- 762	1'7 ×	- 3 $\Delta(\lambda - \Omega)$ - 12 $\Delta(L' - \Omega)$	+ 684 = + 450	+ 151 Partial.
- 647	1'6 ×	- 26 $\Delta(\lambda - \Omega)$ + 8 $\Delta(L' - \Omega)$	+ 567 = + 87	+ 178 Total.
- 430	1'4 ×	+ 23 $\Delta(\lambda - \Omega)$ - 39 $\Delta(L' - \Omega)$	+ 577 = + 587	- 55 Partial.
+ 197	0'7 ×	- 14 $\Delta(\lambda - \Omega)$ + 30 $\Delta(L' - \Omega)$	- 436 = - 401	- 6 Partial.
- 309	1'2 ×	- 8 $\Delta(\lambda - \Omega)$ + 22 $\Delta(L' - \Omega)$	- 227 = - 146	+ 145 Just total (?).
- 128	1'0 ×	- 21 $\Delta(\lambda - \Omega)$ + 34 $\Delta(L' - \Omega)$	+ 018 = - 4	+ 15 Total.

It will be observed that this supposition satisfies not only the two eclipses on which it is based, but also the eclipse of -647 . Of the eclipses which it does not render total, that of -762 is recorded only on the Assyrian eponym canon which records the events of a kingdom, not of a city, and cannot, in consequence, be located with precision. The eclipses of -430 and $+197$ are recorded as not total; and, though I should like a larger magnitude for the eclipse of -430 , I cannot call the magnitude just obtained impossible.

Professor Newcomb finds that, to satisfy Mr. Cowell's five eclipses, it is necessary to assume

$$\Delta\lambda = \Delta L' = 36', \text{ if } \Delta\Omega = 0,$$

corresponding to secular accelerations of $3''\cdot 5$, which means that he divides $36'$ by $(25)^2$. The same rough process gives for the values obtained in this paper

$$\Delta\lambda = \frac{32'}{(25)^2} = 3''\cdot 1, \text{ and } \Delta L' = \frac{19'}{(25)^2} = 1''\cdot 8.$$

But, introducing the time factor, we get

$$\Delta\lambda = \frac{14'}{(20)^2} = 2''\cdot 1, \text{ and } \Delta L' = \frac{8'}{(20)^2} = 1''\cdot 2.$$

This seems much more reasonable than

$$\Delta\lambda = 3''\cdot 5, \text{ and } \Delta L' = 3''\cdot 5.$$

A small refinement might be introduced by substituting Mr. Cowell's centennial motion of the node for Professor Newcomb's. As Mr. Cowell's value exceeds Professor Newcomb's by $8''$, it will be necessary to reduce $\Delta\lambda$ and $\Delta L'$ by $\frac{8''}{25}$ to compensate for the

increase in the centennial motion of the node. This gives us for $\Delta\lambda$, $2''\cdot 1 - 0''\cdot 3 = 1''\cdot 8$, and for $\Delta L'$, $1''\cdot 2 - 0''\cdot 3 = 0''\cdot 9$.

Mr. Cowell, in *Monthly Notices*, lxvi. p. 532, finds from the Ptolemaic lunar eclipses a value of $2''\cdot 6$ for $\Delta L'$, but finally adopts $4''\cdot 1$ on the strength of the solar eclipses. It will be observed that the value just found suits the lunar eclipses very nearly as well as that adopted by Mr. Cowell.

Reducing these corrections to Mr. Cowell's notation (see *Monthly Notices*, lxvi. p. 535) we get with Professor Newcomb's values for the centennial motions $s_F - s_D = +1''\cdot 2$, and $s_D = +0''\cdot 9$. As Professor Newcomb's value for the centennial motion of $F - D$ exceeds Mr. Cowell's by $8''$, while his value for the centennial motion of D exceeds Mr. Cowell's by $4''$, the substitution of Mr. Cowell's centennial motions for Professor Newcomb's would give

$$s_F - s_D = +1''\cdot 2 - \frac{8''}{25} = +0''\cdot 9, \text{ and } s_D = +0''\cdot 9 - \frac{4''}{25} = +0''\cdot 7.$$

The resultant corrections to Professor Newcomb's secular terms are therefore quite small. The value obtained for $s_F - s_D$ suggests that there is in existence either a very small acceleration of the Sun, or some very small, hitherto unexplained, retardation of the node; but when we allow for the possibility of errors in the centennial motions, and for the slight disturbance in the results that might be occasioned by an error in the position of perigee, it seems impossible to affirm such small corrections on the basis of so few eclipses. It would appear, therefore, that these eclipses do not by themselves prove the existence of a secular acceleration of the Sun, though they are consistent with an acceleration of about $1''$ a century.

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1909 March 19.

Errata in Mr. Fotheringham's Papers.

Page 19, line 21, for 53^s.50 read 44^s.63.

Page 27, line 43, for 10'61 read 10'77.

Page 206, lines 27 to 30, the values of G should be increased by 180° , and the values of $\sin g$ should be positive instead of negative.

Errata in Annual Report, vol. lxix.

Note on Minor Planets, p. 293, line 15 from bottom:—delete "BL with 198 Ampella" and substitute "an image on a photograph taken by Metcalf 1908 Jan. 30 was announced as belonging to BL, but really belonged, not to this planet, but to 198 Ampella."

Also the three planets announced as discovered 1908 Jan. 24 (p. 292) were really discovered on Jan. 23.

Note on Variable Stars, p. 310:—In 3rd. paragraph, line 1, and 4th. paragraph, line 7, for vol. I. read vol. l.